MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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W. H. MAW, Esq., PRESIDENT, in the Chair.

Rev. Walter Briscombe, Hillam Royd, Abbey Park Road, Grimsby ;
Arthur Stanley Eddington, B.Sc., B.A., Royal Observatory, Greenwich ; and
Dr. Edalji Manekji Modi, F.C.S., &c., Sleater Road, Bombay,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

John de Fenton, Ph.B., Secretary, Seymour Avenue, Parktown, Johannesburg, South Africa (proposed by R. T. A. Innes) ;
John Grigg, The Observatory, Thames, New Zealand (proposed by W. Steadman Aldis) ; and
Robert Leetham Jones, M.A., Barrister-at-law, 3 King's Bench Walk, Temple, E.C. (proposed by Fredk. W. Crowe).

Professor Julius Franz, Observatory, Breslau, Germany, was proposed by the Council as an Associate of the Society.

Ninety-four presents were announced as having been received since the last meeting, including, amongst others :—

G. W. Hill, collected mathematical works, vol. ii., presented by the author ; Lowell Observatory Annals, vol. iii., presented by Professor Lowell ; Telegraphic determinations of longitude,

1888–1902, made under the direction of Sir W. H. M. Christie; and eighteen charts of the Astrographic Chart of the Heavens, presented by the Royal Observatory, Greenwich; two lantern slides of spectroheliographs of the Sun taken at Kodaikánal Observatory, presented by the Director; six large transparencies from photographs of the Milky Way taken by Professor E. E. Barnard at Mount Wilson, California, presented by the Yerkes Observatory.

A Tentative Explanation of the Apparent Secular Acceleration of the Earth's Orbital Motion. By P. H. Cowell.

I have recently shown that, in order to satisfy the ancient eclipses of the Sun and Moon, it is necessary to assume that the mean longitude of the Moon contains a term $+11'' T^2$, and that the mean longitude of the Sun contains a term $+4'' T^2$.

These two arbitrary assumptions satisfied six solar eclipses. It was inconceivable that this could be a mere coincidence. Moreover, in the lunar eclipses of the Almagest, if we try to satisfy the records by assuming unknown secular accelerations for both Sun and Moon, we are led to precisely the same conclusions. I have felt it impossible to doubt that the records are trustworthy, and that no tables of the Sun and Moon will be completely satisfactory that fail to agree with those records.

There is still, however, some latitude of interpretation left. The eclipses determine at certain times the relative positions of the Sun, Moon, and node of the Moon's orbit. Two relations in fact exist between the four quantities, the position of the Sun, the position of the Moon, the position of the node, and the time.

The history of the subject is briefly as follows:

Halley discovered the secular acceleration of the Moon, Laplace showed that the changes produced by the planets in the eccentricity of the Earth's orbits would produce effects of the kind noted by Halley, Adams first correctly calculated the secular acceleration of the Moon's mean motion, and Professor Brown has given (*Monthly Notices*, lvii. p. 348) the following numerical values of the secular accelerations per century (measured from a fixed or uniformly moving departure point):

For the mean motion	$+5''.91$
For the node	$+6''.56$

From the ancient eclipses I obtained as observed values of the secular terms:

For the distance from the Sun to the Moon ...	$+7''$
For the distance from the node to the Sun ...	$-2''.4$

The excess of the observed over the theoretical secular accelerations is therefore :

From the Sun to the Moon	+1″
From the node to the Sun	+4
From the node to the Moon	+5

The mean motions of the Sun and Moon can only be obtained from observation, whereas the mean motion of the node can be obtained from theory ; as the theoretical motion of the node agrees with observation at the present time, it seems more reasonable to attribute the above excess of observed over theoretical secular accelerations to the Sun and Moon rather than to the node.

The observed excess to be explained is therefore :

For the Moon	+5″
For the Sun	+4

Since the time of Lagrange (who, by the way, refused to believe in the secular acceleration of the Moon as an observed fact until this idea had occurred to him) it has been considered permissible to assign any secular acceleration of the Moon to a lengthening of the day. If the day increases in length by one part in a million, the mean motion of the Moon would appear to increase by one part in a million ; so also would the Sun's mean motion ; but the numerical increase would only be one thirteenth part as large as for the Moon.

Mr. Eddington reminded me that Sir George Darwin has pointed out that the principle of conservation of angular momentum implies that, if the Earth is losing its axial rotation, the Moon's orbit must be at the same time expanding, and its angular motion diminishing.

Let N, n denote the angular velocities of the Earth's rotation and of the Moon's revolution, and let δN, δn denote the variations of these quantities in a century ; then the apparent secular acceleration of the Moon is

$$\tfrac{1}{2}\left(-\frac{\delta \mathrm{N}}{\mathrm{N}} + \frac{\delta n}{n}\right) \text{ times its mean motion}$$

and the apparent secular acceleration of the Sun is

$$\tfrac{1}{2}\left(-\frac{\delta \mathrm{N}}{\mathrm{N}}\right) \text{ times its mean motion}$$

where the factor $\frac{1}{2}$ is introduced, because secular accelerations are defined as the coefficients of T^2 in the corresponding quantity, instead of $\frac{1}{2}\mathrm{T}^2$.

δN, δn are both negative quantities, and it is easily shown that $\frac{\delta n}{n}$ is a very large fraction of $\frac{\delta N}{N}$, and hence that the ratio of the Moon's secular acceleration to its mean motion is a small fraction of the corresponding ratio for the Sun if tidal friction be the cause.

This is precisely the inference from ancient eclipses :

For the Sun the observed ratio is

$$4'' \text{ to } 100 \text{ revolutions, or } 3 : 10^8$$

For the Moon (excess over the Laplace effect)

$$5'' \text{ to } 1336 \text{ revolutions, or } 3 : 10^9$$

To fit in with these numbers it is necessary to show that

$$\frac{\delta n}{n} = \frac{9}{10}\frac{\delta N}{N}$$

so that

$$-\frac{\delta N}{N} + \frac{\delta n}{n} \text{ may be one tenth of } -\frac{\delta N}{N}$$

Let I be the amount of inertia of the Earth, m, a, e the mass, mean distance, and eccentricity of the Moon ; then the moment of angular momentum is

$$I \,.\, N + mna^2(1-e^2)^{\frac{1}{2}}$$

The principle of the conservation of angular momentum will only give one relation between δN, δn and δe ; but this relation is sufficient to show that $\frac{\delta n}{n}$ must be a large fraction of $\frac{\delta N}{N}$, though not necessarily nine tenths.

Differentiating and neglecting the effect of solar tides,

$$I\delta N + mna^2(1-e^2)^{\frac{1}{2}}\left[-\tfrac{1}{3}\frac{\delta n}{n} - \frac{e\delta e}{(1-e^2)}\right] = 0$$

or

$$-k\frac{\delta N}{N} + \tfrac{4}{3}\frac{\delta n}{n} + 4\frac{e\delta e}{1-e^2} = 0$$

where k is approximately unity, if we assume that the Earth's moment of inertia may be calculated as if it were homogeneous.

In this equation put $k = 1$, $\delta e = 0$

then

$$\frac{\delta n}{n} = \tfrac{3}{4}\frac{\delta N}{N}$$

which proves, at any rate, that $\frac{\delta n}{n}$ is a large fraction of $\frac{\delta N}{N}$. Also the ratio is increased if a positive value is assigned to δe.

In the same equation substitute the observed values

$$\frac{\delta N}{N} = -6 : 10^8 \qquad \frac{\delta N}{N} - \frac{\delta n}{n} = -6 : 10^9$$

(the factor 2 being introduced) then

$$\delta e = (\tfrac{6}{5} - k)\frac{2{\cdot}7}{10^7} = 0''{\cdot}01 \text{ per century if } k = 1$$

The supposition that the observed accelerations follow from the theory does not therefore conflict with the observed eccentricity.

On the other hand the transits of *Mercury* exhibit some slight evidence against this hypothesis, but perhaps not sufficiently important to destroy it.

Two conclusions follow from this paper :

I. It is absolutely wrong to assign an arbitrary secular acceleration to the Moon and none to the Sun, and to justify this course by the supposed action of the tides. This has been the practice of the *Nautical Almanac* since 1883.

II. On the hypothesis contained in this paper the rate at which the day is increasing is six parts in 10^8 or $0^s{\cdot}005$ per century. This is about ten times as large as previous estimates.

Observations of the Magnitudes and Position of Nova Geminorum
By E. E. Barnard.

This star was discovered by Professor H. H. Turner on a photograph taken at Oxford on 1903 March 16.

My work on the Nova has consisted of comparisons of its light with that of certain stars and measurements of its position relative to small stars near it. I have also watched the star for any change of focus due to the usual changes in the spectrum of a Nova. The star was at first of a strong red colour, but this soon faded out and left it colourless. At first there did not seem to be any difference in the focus from that of an ordinary star, but in the latter part of April there seemed to be some slight difference, the star perhaps coming to a focus slightly outside of that for an ordinary star (see *Ap. J.* xvii. 1903, p. 376). The notes show that on 1903 August 31 the Nova was whitish and hazy. On 1903 September 21 at $15^h\ 45^m$, with a magnifying power of 700 diameters, the Nova was decidedly out of focus when compared with the other stars. Careful measures

of the focus were made with reference to a 10th magnitude white star preceding.

Nova ...	...	2·54 inches (5 obs.)
10^m star ...	...	2·25 „ (6 „)
Difference ...		0·29 inch

The focus for the Nova was therefore 0·29 inch further from the object-glass than for a star. On 1905 November 25, by slipping the eyepiece back and forth, the focus for the Nova seemed to be quite outside of that for a star, but the seeing was too bad to make any measure of it. Speaking of the focus of the Nova, there was one thing about it that I have never seen in the case of any other star. In experimenting with the focus on 1903 March 30 I found that the star really had two distinct foci, both of which gave sharp definite images, one of about the $8\frac{1}{2}$ magnitude, of a reddish-yellow colour at the ordinary focus for a star, the other of the 10th magnitude 0·39 inch further out from the object-glass and of a beautiful crimson colour and $0''{\cdot}1$ in diameter. This last image was really the best defined of the two. In the first case there was an inconspicuous crimson halo about the star, and in the second a pale greyish-blue halo $3''{\cdot}8$ in diameter. Either of these images would readily have been taken for the true image of the star. On April 6 this crimson image was still present though not so strong and definite. On April 27 it had entirely disappeared. The out-of-focus image of the Nova then resembled that of an ordinary star. On account of cloudy weather it had not been possible to tell just when this change took place. I understand that this crimson image was due to the strong $H\alpha$ line in the spectrum of the star. When the other stars were examined outside and inside the focus no such peculiarity was seen.

Mr. Parkhurst had secured a photograph, 1903 February 21, with the 2-foot reflector of the region of the Nova. On this plate a faint star was shown close to the position afterwards occupied by the Nova; this small star, however, was near the edge of the plate where spherical aberration made it difficult to measure with accuracy. It was thought that this might be the Nova before it became bright. When the Nova had faded sufficiently, however, to permit one to see a faint object near it, I found on the morning of 1904 September 1 that the small star itself could be seen close preceding the Nova. Measures of the position of this star with respect to the Nova are given in the list of measures that follows.

A few of these measures were given in *Ap. J.* xvii. 1903, pp. 301, 302. In those measures, however, the magnitudes assigned the small stars were only rough, simply to aid in the identification of the stars. On two dates since then I have carefully estimated their magnitudes. This is rather important because these estimations are on the same scale that I have used throughout

in my measures of the small stars in the globular clusters. These estimations are given below :

Direct Estimates of the Magnitudes of Small Stars near Nova Geminorum.

	1903 Oct. 26. m	1904 Dec. 5. m	Mean. m		1903 Oct. 26. m	1904 Dec. 5. m	Mean. m
1	13·0	13·5	13·2	5	12·8	12·6	12·7
2	13·8	14·2	14·0	6	12·8	13·4	13·1
3	15·0	14·5	14·7	7	...	...	15·1
4	12·8	12·5	12·7				

The magnitudes of five of these stars have been determined photometrically by Mr. Parkhurst. They are :

	m		m
1	13·29	6	13·11
3	14·84	7	15·27
5	13·00		

In *Ap. J.* xvii. 1903, pp. 376, 377, I have given my determinations of the magnitudes of the Nova from 1903 March 27 to May 19. To make the present paper more complete I will give here these results, which depend on stars whose magnitudes were photometrically determined by Mr. Parkhurst. They are based on the Harvard system. The observations of May 23 and 27 were not printed in the *Ap. J.*

Observed Magnitudes of Nova Geminorum.

1903.	Central Standard Time. h m	Mags.	1903.	Central Standard Time. h m	Mags.
March 27	12 30	8·00	April 26	8 30	9·81
29	9 0	8 81	27	8 30	9·96
30	9 0	8·82	28	8 30	10·18
31	7 20	8·84	May 3	8 20	9·80
April 3	8 0	8·96	6	8 10	9 80
4	7 40	9·04	7	9 10	9·87
6	9 0	9·15	8	8 30	9·77
7	10 20	9·22	10	8 10	9·77
9	8 0	9·12	18	8 30	10·02
16	7 40	9·12	19	8 30	10·07
17	9 0	9·20	23	8 30	10·05
22	7 50	9·20	27	8 50	10·05
24	8 15	9 92			

At the observation of March 27 the star was very low and the estimated magnitude is unreliable. A direct estimate made it 8·2 magnitude. It was estimated to be 0·2 magnitude less than B.D. + 29°, 1342.

Magnitudes of the Nova when Faint.

1903.	h	m	m	
Aug. 31	16	25	12±	Rough
Sept. 1	16	0	11±	estimate
Oct. 13	14	30	12·3	
26	14	30	12·3	
27	14	0	12·3	
Dec. 14	11	10	12·3	
29	15	30	12·3	
1904.				
April 19	9	50	12·2	
May 2	7	55	12·1	
3	7	50	12·1	
Oct. 1	14	45	13·6	
1904.				
Oct. 17	14	0	13·7	
Nov. 12	13	40	13·8	
21	17	40	14·4	
26	11	40	14·5	
Dec. 5	10	45	14·3	
1905.				
Oct. 31	17	0	14·5	
Nov. 25	15	40	14·9	
Dec. 23	14	30	14·8	
30	13	10	14·4	
1906.				
Feb. 27	11	30	14·8	

These determinations depend upon the magnitudes of the stars given in the measures.

The Nova is now but little brighter than the small star close preceding it (the one thought to be the Nova on the photograph of 1903 February 21). With bad seeing the light of the two stars mixes, and this makes any comparison of the light of the Nova somewhat uncertain. The last observation seemed to be a good one.

The measures of the position of the Nova do not seem to show any certain motion in that object. Nor does there seem to be any indication of a parallax.

Nova Geminorum.

Nova and 1.

			°	″
1903	Mar.	30	104·82	32·21	
	Apr.	4	105·22	32·52	
		6	105·03	32·28	
		7	104·97	32·38	
		28	104·71	32·66	
	Aug.	31	104·25	32·10	
	Sept.	21	104·49	32·52	
		28	104·35	32·36	
1904	Oct.	1	104·43	32·27	
		17	104·38	31·98	
	Nov.	12	104·94	32·08	
		Mean	104·60	32·30	
			°	″	
1905	Nov.	25	104·40	32·04	
	Dec.	23	104·98	31·44	Very difficult.
		26	103·86	32·22	
		30	105·10	31·89	
		Mean	104·59	31·90	

There seems to be a decrease of distance between Nova and star 1. This change is not certain, and further measures will be made to settle the question of motion. If the motion is in the Nova it ought to tell in the distances of stars 7 and 8 also, and in the position angles of the other stars.

Nova and 4.

1903	Mar.	30		196°·15	84″·93	
	Apr.	4		196·30	85·15	
		6		196·29	84·99	
	Aug.	31		196·03	85·07	
	Sept.	21		196·14	84·99	
		28		196·23	84·96	
1904	Oct.	1		196·14	...	Clouds.
		17		196·29	85·06	
	Nov.	12		196·52	85·07	
	Dec.	5		196·27	84·96	
			Mean	196·23	85·02	
1905	Nov.	25		196°·61	85″·06	
	Dec.	23		196·87	84·79	
		26		196·24	84·93	
			Mean	196·57	84·93	

Nova and 2.

1903	Mar.	30		169°·20	45″·66	
	Apr.	6		168·78	45·61	
		27		168·83	46·31	Uncertain.
	Sept.	21		168·72	45·73	
		28		168·77	45·54	
1904	Oct.	1		169·52	45·24	
		17		169·43	45·54	
	Dec.	5		169·34	45·80	
			Mean	169·07	45·68	
1905	Nov.	25		169°·36	45″·63	
	Dec.	23		169·69	45·25	
		26		169·08	45·70	
			Mean	169·38	45·53	

Nova and 6.

1903	Mar.	30	16°98	102″81
	Apr.	4	17·45	103·00
		6	17·05	102·86
		7	17·31	102·78
	Sept.	21	16·89	103·13
		28	16·81	102·94
	Oct.	13	17·09	102·93
1904	Oct.	17	17·22	102·76
		Mean	17·10	102·90

Nova and 5.

1903	Mar.	30	313°86	99″87
	Apr.	4	313·90	99·94
		6	313·87	99·86
	Sept.	28	313 78	100·11
	Oct.	13	313·80	99·87
		19	313·94	100·03
		26	313·72	99·98
1904	Oct.	17	313·65	99·78
	Nov.	12	313·51	100·05
	Dec.	5	313·63	100·05
		Mean	313·77	99·95

Nova and 3.

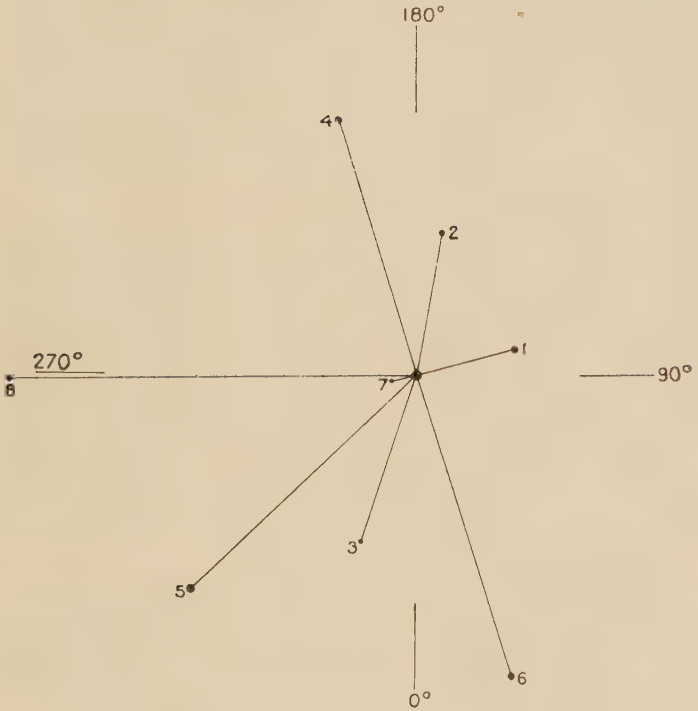
1903	Mar.	30	341°61	55″69	
	Apr.	27	341·45	...	Too difficult for distance.
	Oct.	26	341·45	55·64	
		Mean	341·50	55·66	

Nova and 7.

1903	Aug.	31	284°35	7″83
	Sept.	28	281·61	8·03
	Oct.	26	283·09	7·94
		27	283·77	7·60
		Mean	283·20	7·85

Nova and 8 ($14^m \pm$).

1903 Oct.	26	270°·90	131″·76
	27	270·83	131·27
Nov.	16	270·98	131·23
	Mean	270·90	131·42



The accompanying chart of these stars will make their identification easy, even if the Nova should entirely disappear.

High-level Chromospheric Lines and their Behaviour in Sun-spot Spectra. By A. Fowler.

About a year ago I communicated to the Society an account of some spectroscopic observations of the great sun-spot of 1905 February, and of the prominences observed when the spot was on the Sun's limb.* An attempt was then made to distinguish

* *Monthly Notices*, vol. lxv. p. 513.

between high-level and low-level lines in the spectrum of the disturbed chromosphere, and it was pointed out that the high-level lines were not among those intensified in the spot, while the lines common to spots and prominences were chiefly those of iron, chromium, and calcium which appear as strong Fraunhofer lines. Since then I have made numerous observations which furnish additional data for such a comparison ; and although the records are not yet so complete as might be desired, the following notes may serve a useful purpose by drawing the attention of observers to some points requiring further investigation, while spots and metallic prominences are still relatively numerous. The Evershed two-prism solar spectroscope has been employed throughout, chiefly in conjunction with the 6-inch refractor at the Royal College of Science, South Kensington, but during 1905 August, in conjunction with a $4\frac{1}{2}$-inch refractor at Castellon, Spain.

Continued attention has been given to the distinction between high- and low-level lines for the reason that Professor Young has not always made this distinction in his well-known list of chromospheric lines, and sufficiently good eclipse photographs of the less refrangible parts of the spectrum are not yet available. When observing the spectrum of a metallic prominence with a tangential slit, the high-level lines are recognised as such by their relatively great length, while the low-level lines are usually very short. In each case the lines are of various intensities, and some of the long lines are faint as compared with the brighter of the short ones. The longest lines of all are, of course, those of hydrogen and helium, but the term "high-level" may be conveniently understood to include all lines which show any considerable extension.

The lines observed on different occasions during the last two years have always been the same, so far as it was possible to determine their positions, and their relative intensities have been nearly constant ; those at 5169·2, 5018·6, and 4924·1, however, have shown a certain amount of variation when compared respectively with b_1 and the helium lines 5015·7 and 4922·1.

All the long lines which have yet been noted, but not all the short ones, occur in Young's list, though not many of them are designated as high-level lines. The short lines, so far as they have been identified with terrestrial spectra, correspond with arc lines which are not enhanced on passing to the spark. The long lines, excluding the exceptionally long ones of hydrogen and helium, may be considered as of two varieties : one including the D lines of sodium and the *b* lines of magnesium, which are strong lines both in arc and spark ; the other consisting of enhanced lines so far as their origin can at present be traced. Most of these enhanced-line identifications are due to Sir Norman Lockyer,* but my own investigations indicate that at least four

* *Phil. Trans.* vol. cxcvii. A, p. 151.

additional lines are to be assigned to iron, one to titanium, and one to chromium. Full details as to all the lines recorded are reserved until more observations have been secured, but the wave-lengths and intensities of the high-level lines, excluding those of hydrogen, helium, sodium, magnesium, and barium, are given in a table which follows.

The phenomena observed in these disturbances of the chromosphere seem to indicate, as suggested in my former paper and in accordance with the views of Sir Norman Lockyer, that the chromospheric "layers" are elevated without any great disturbance in their order. On this point Lockyer writes: * "If the Sun is quiet, or if we observe any particular part of it at any time at which it is not agitated, the layers visible at that time, few in number, are usually concentric, but the moment there is any agitation in the subjacent photosphere the lower layer shoots up into the next layer above it; the next shoots up into the one next above that; and so on; . . . from the very lowest layer to the upper hydrogen one the layers are made to obey this same impulse, and bulge up like so many domes on that part of the Sun which is being violently agitated."

The long lines, whatever their brightness, are more frequently observed than the shorter ones, because it takes a less violent disturbance to bring them into view. The shorter lines, notwithstanding their great brightness in some cases, are only seen on the comparatively rare occasions when the disturbance is sufficiently great to elevate the low-lying flash stratum to such a height that it can be differentiated from the photospheric spectrum. This, at all events, seems to provide a simple explanation of the appearances observed.

It thus appears probable that the metallic prominences yield occasional opportunities of observing the flash spectrum in addition to those furnished by eclipses, and since high dispersion may be employed the positions of the various lines may be determined with the accuracy essential for proper comparison with spot spectra and for purposes of identification.

The additional observations which have been made confirm generally my previous statement as to the relative behaviour of high-level and low-level lines in the spectra of spots, and Mr. W. M. Mitchell, of Princeton, states that his observations are in complete agreement upon this point.† Mitchell further observed that some of the high-level lines, so far from being intensified in the spots, are thinned or obliterated. My own observations have confirmed those of Mitchell in this respect and indicate additional examples of the same behaviour. Some of these, and others which I have confirmed by visual observations, are also indicated by the recent photographic records of Hale and Adams.‡

* *Chemistry of the Sun*, p. 169.
† *Astrophys. Journ.* vol. xxii. July 1905, p. 40.
‡ *Ibid.* vol. xxiii. January 1906, p. 11.

The extent of this thinning or obliteration of lines is possibly somewhat variable from spot to spot, but the varying amount of scattered light, depending on the state of the sky, makes it difficult to ensure certainty on this point. In an observation of the large spot in the south preceding quadrant on 1906 March 27, however, the thinning of the lines appeared to be unusually distinct, and on one or two occasions the line 5169·2 has been seen clearly bright over the umbra. Some of these lines are components of close doubles in the solar spectrum, and the thinning or obliteration then reveals itself as a notch on one side of the compound line where it is crossed by the spot. The iron line 5169·22 with its companion 5169·07 due to iron is one of the best examples; my attention was first drawn to it by Mitchell's paper, and I have since recognised other similar cases.

Particulars as to the approximate intensities of thirty high-level chromospheric lines and of their behaviour in spots are given in the following table. The wave-lengths and solar intensities are from Rowland's tables, and the enhanced-line identifications are Lockyer's, except for those marked F, which result from my own experiments. The prefix "p," as employed by Lockyer, is an abbreviation for "proto," and indicates that the line is relatively enchanced in passing from the arc to the spark spectrum. Under the heading "Spots" the numbers are intensities on the solar scale, except those of Mitchell, which are relative numbers such that −5 indicates complete obliteration. The general behaviour of the lines tabulated suggests that those which are still unidentified will be found to correspond with enhanced lines.

Wave-length.	Orig'n.	Intensity and Character in Chromos. (F)	Intensity in Sun. (R)	Spots. — (F)	Hale and Adams.	Mitchell.	Remarks.
4924·11	*p* Fe	25 *l*	5	Reduced to 3	Out of range	...	...
5018·63	*p* Fe	25 *l*	4	Much reduced	...	5	(1)
5154·24	*p* Ti	15 *l*	2	Reduced to 1	...	...	...
5169·22	*p* Fe	40 *l*	4	Nearly obliterated	·07, ·22 } weak on red edge	−4	(2)
5186·07	*p* Ti_F	10 *l–s*	2	Reduced to 1	...	−3	...
5188·86	*p* Ti	15 *l–s*	2	Reduced	·86, ·02 } hazy towards violet	2	(3)
5197·74	*p* Fe_F	25 *l*	2	Reduced to 0	Reduced to (0–1)	...	...
5226·71	*p* Ti	20 *l–s*	2	Reduced to 1	...	Obliterated once	...
5234·79	*p* Fe_F	25 *l*	2	Reduced to 0	Reduced to 1	−3	...
5237·49	*p* Cr_F	10 *l*	1	„ „	Reduced to (0–1)	−3	...
5264·98	...	15 *l*	0	Obliterated	Reduced to 00	...	...

Wave-length.	Origin.	Intensity and Character in Chromos. (F)	Intensity in Sun. (R)	Spots. — (F)	Spots. Hale and Adams.	Spots. Mitchell.	Remarks.
5276·17	*p* Fe	35 *l*	3	Not clearly affected	...	Generally not affected	(4)
5284·28	...	25 *l*	1	Nearly obliterated	...	...	(5)
5316·79	*p* Fe	45 *l*	4	Much reduced	...	...	(6)
5325·74	...	15 *l*	2	Reduced to 0	...	...	(7)
5336·97	*p* Ti	15 *l–s*	4	Reduced to 2	...	...	(8)
5363·06	*p* Fe_F	30 *l*	3	Much reduced	...	...	(9)
5381·22	*p* Ti	10 *l*	2	Reduced to (0–1)	...	...	(10)
5425·46	...	30 *l*	1	Nearly obliterated	Reduced to (0–1)	−4	...
5527·03	*p* Sc	20 *l*	3	Reduced to 2	...	...	(11)
5535·06	?*p* Fe_F	35 *l*	2	Reduced to 1	Reduced to 1	...	...
5991·60	...	15 *l*	2	Nearly obliterated	Out of range	...	...
6238·60	...	15 *l*	2	,, ,,	,, ,,	−4	...
6247·77	...	20 *l*	2	,, ,,	,, ,,	−4	...
6347·31	...	25 *l*	2	Obliterated	,, ,,	−4	...
6369·68	...	10 *l*	0	...	,, ,,	...	(12)
6371·57	...	15 *l*	1	Nearly obliterated	,, ,,	...	(13)
6432·89	...	25 *l*	1	,, ,,	,, ,,	−4	(14)
6456·60	*p* Fe_F	20 *l*	3	Obliterated	,, ,,	−4	...
6516·31	...	30 *l*	2	...	,, ,,	...	(15)

Remarks.

1. A close double in Sun, Ni 5018·46 (1) and *p* Fe 5018·63 (4). The line is distinctly notched on red side.

2. A close double in Sun, Fe 5169·07 (3) and *p* Fe 5169·22 (4). Notched on red side, as first described by Mitchell.

3. A close double in Sun, *p* Ti 5188·86 (2) and Ca 5189·02 (3). Notch on violet side observed with difficulty, probably on account of widening of Ca line.

4. A very close double in Sun, *p* Fe 5276·17 (3) and Cr (?) 5276·24 (2). The widening of the chromium line possibly obscures the reduction of the *p* Fe line.

5. Rowland gives origin as Ti, but the line does not appear in Hasselberg's table. It may be an enhanced line, but the dispersion of my photographs does not clearly separate it from adjacent lines.

6. This is "1474 K." The reduced intensity is clearly seen in the reproduced photograph given by Hale and Adams. Young has also noted that the line is "distinctly weakened and sometimes reversed." There are closely adjacent lines at 5316·91 (0) and 5316·96 (2).

7. Young gives the wave-length of the chromospheric line as 5325·4. The reduced intensity of the dark line is seen in Hale and Adams' photograph.

8. The line is distinctly weakened in Hale and Adams' photograph.

9. A close double in Sun, Fe 5362·94 (1) and *p* Fe 5363·06 (3) The line is notched on the red side in spots.

10. Rowland attributes solar line to Fe.

11. Lockyer attributes this line to *p* Sc in *Roy. Soc. Proc.* vol. lxix. p. 359.
12. Rowland attributes solar line to iron. In my instrument it is too feeble in the Sun to determine whether it is reduced in spots or not, but it is certainly not intensified.
13. Rowland attributes solar line to iron.
14. Rowland gives Fe ? for this line.
15. This line is difficult in my instrument on account of adjacent line at 6516·8; it is certainly not intensified in spots, and I believe that it is much reduced in intensity.

The general result of the comparison is to show that a Fraunhofer line agreeing in position with a high-level chromospheric line, if it be of "proto-metallic" origin, is especially liable to be thinned or obliterated in the spectra of spots. In many cases there is a sufficient agreement among the different authorities as to their behaviour in spots, while for others further independent confirmation is desirable. As indicated in the remarks, however, some of the reduced lines which are noted only in my own observations are confirmed by the photograph of Hale and Adams, though they are not shown in the tables of these observers. Many of my observations have also been confirmed by my assistant, Mr. Shaw.

By way of explanation it might be supposed that the vapours producing the enhanced lines in the chromosphere and the corresponding lines in the Fraunhofer spectrum are chiefly restricted to the higher levels, and that their thinning or obliteration in spot spectra is due to the actual withdrawal of the more elevated vapours from the region directly over a spot. On the other hand, the occasional brightening of some of the lines suggests that the chromosphere retains its continuity over the spots, and is sometimes of more than ordinary brightness. It is possible, therefore, that the reduced intensity is to be accounted for by the umbra not providing a sufficiently luminous background to strongly exhibit the absorption of the high-level vapours, while the lines themselves are rarely of sufficient intensity to appear bright.

Observations are somewhat conflicting as to the continuity or otherwise of the chromosphere over spots. Young writes:* "Respighi asserts (and the most careful observations we have been able to make confirm his statement) that, as a general rule, the chromosphere is considerably depressed immediately over a spot. Secchi, however, denies this." Further investigation of this matter is greatly to be desired.

It should be remarked in conclusion that there are many lines which are weakened in spots besides those to which reference has been made, but neither enhanced lines nor high-level chromospheric lines corresponding with them have yet been recorded. Many of these are probably lines of the "widened and weakened" type described by Mitchell, which, with moderate dispersion, are not clearly distinguishable from thinned or obliterated lines.

* *The Sun*, 1901 ed., p. 216.

The most definite conclusion indicated by the observations is that enhanced lines of iron, chromium, and titanium appear as high-level lines in the chromosphere, and that the corresponding Fraunhofer lines are generally enfeebled in the spectra of sun-spots.

[Note, April 28.—In *Bulletin* No. IV. of the Kodaikánal Observatory, received since the above paper was read, Mr. Michie Smith records two observations of the obliteration of 6247·77, one of 6347·31, and one of a line at 6084·32. It is interesting to note that the latter is described by Young as a faint high-level chromospheric line, though it is not yet included in my list.]

The Partial Eclipse of the Sun 1906 *February* 22, *observed at Adelaide.* By Sir Charles Todd, K.C.M.G., F.R.S.

(*Extract from a letter to A. M. W. Downing.*)

We observed the eclipse as follows :—

	h	m	s	
First contact	5	58	58	Considered good.
Last contact	6	58	42	Not good, Sun too low and limb very badly defined.

Adelaide standard times (9^h 30^m East) of February 23.

Observatory, Adelaide:
1906 *February* 26.

On the Part played by Contrast in the Appearance of the Dusky Areas of Mars. By E. M. Antoniadi.

In his valuable "Observations of *Mars*, 1903" (*Monthly Notices*, vol. lxv. No. 8, June 1905, p. 840), Major P. B. Molesworth, R.E., has written that if my theory of contrast be "pressed to extremes it becomes a most dangerous argument. If we carry it to its logical conclusion we shall have a *Mars* deprived of all 'nuances' of light and shade, both light and dark markings being broad masses of uniform tone unrelieved by any half-tones or delicate shading."

Now in reply to this it is perhaps unnecessary to remark that any physical hypothesis is destined to account for some definite phenomena, and no others. The utility of pressing a theory to extremes is therefore questionable to my mind. At the same time Major Molesworth's "logical conclusion" from

the contrast hypothesis, that it would give absolute uniformity to the dusky areas of *Mars*, is gratuitous, and, moreover, in direct antagonism to facts, since contrast, as shown by observation and experiment, tends to intensify the edges of *all* duskinesses, and this quite independently of their albedo.

Major Molesworth has obviously misunderstood the expression "rough evenness of tint" * which I have applied to some dusky areas of the planet. The word "evenness" was used only in opposition to the unevenness resulting from Signor Schiaparelli's sea-girding "canals," which I have attributed to contrast.† That I never meant for a second that the grey markings of *Mars* were even in tint is evident (*a*) from the above expression, "rough" evenness implying, of course, a somewhat checkered structure of the shadings; (*b*) from the statement made in the very paper in which my contrast theory was framed in 1903, and running thus: "That the intensity of the grey spots is far from being uniform no one will deny"; ‡ and (*c*) from my numerous published maps of *Mars* from 1892 to 1903, and particularly the small chart accompanying the 1903 considerations on contrast, in which the intensity of the shadings is shown to vary most unmistakably, and by delicate gradations, from the faintest half-tones to the darkest areas.§ Such converging evidence goes far to establish beyond doubt the uselessness of the attempt to invalidate the contrast hypothesis by straining it to conclusions of a baseless and inaccurate character.

But Major Molesworth raises a far more interesting question when doubting that the intervention of contrast and of fatigue of the eye can be nugatory to eye training during the successive apparitions of *Mars*.‖ Examples, however, tending to prove the reverse are numerous. In 1890, for instance, Signor Schiaparelli's persistent efforts had reached, after several weeks of real eye-training, what looks like the culminating point in the visibility of delicate objective detail when Sinus Sabæus appeared with a wavy, irregular outline and a fairly uniform intensity (fig. 1). But what, may we ask, was the result of further observation? The disappearance of detail, the brightening of the inner part of the Strait, and the intensification in two very dark perfectly straight lines of the N. and S. borders (fig. 2).

Inasmuch as Major Molesworth considers that eye-training goes on steadily improving during a given apparition, he must logically admit the rude form of 1890 June 20 as giving a

* *English Mechanic*, 1903, No. 2016. † *Knowledge*, 1903, pp. 247, 248.
‡ *Ibid.* p. 248. § *Ibid.* p. 249.
‖ Here Major Molesworth asks if contrast is applicable to the duplicity of *Jupiter's* belts. These I consider objectively double in the immense majority of cases. There can be no comparison between the solid globe of *Mars* and gaseous *Jupiter*, whose rotation period varies with latitude. On *Saturn* contrast gives rise to the "square shoulder" appearance and to Dr. Terby's white spot on the rings.

truer view of Sinus Sabæus than that of May 16, the atmospheric definition being excellent in both cases and the probability of objective change on such a vast scale being out of question. But the evidently artificial character of such an assumption is avoided by the theory of contrast; considering fig. 1 to represent the best view of Sinus Sabæus, and the irregularly indented outline to look like the natural edge either of a vegetation tract

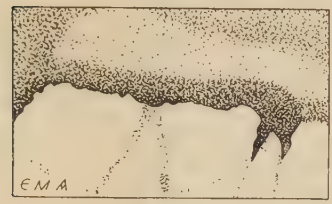


Fig. 1.
May 16.

Fig. 2.
June 20.

Appearances presented by Sinus Sabæus in 1890 according to Signor Schiaparelli.

or of water, or a combination of both, fig. 2 corresponds to the vagaries of an exhausted retina, since, according to contrast, fig. 1 will appear like fig. 2 after a very prolonged fixity of gaze.* I have, however, an open mind; and the day when Major Molesworth will have succeeded in demonstrating that the amelioration of telescopic seeing by eye-training involves the positive *loss* of planetary detail he will have established his contention on a strong basis.

Observations of Comet a 1904, *from Photographs taken with the* 30-*inch Reflector of the Thompson Equatorial and the* 13-*inch Astrographic Refractor at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The following positions of Comet *a* 1904 were obtained from photographs taken with the 30-inch reflector, except that on 1904 April 17, which was taken with the 13-inch astrographic refractor. From two to four exposures were made on each plate, the duration being 3^m in 1904 April and May, 2^m in June, afterwards increasing to 7^m in October. The last photograph, on 1905 May 8, has two exposures of 20^m each.

* I attribute to a fatigue of the retina the disappearance of the faint half-tones of the N. hemisphere of *Mars* from the charts of Signor Schiaparelli and Mr. Lowell. The tired eye sees better the outlines than the central regions of faint duskinesses.

The plates were measured in the astrographic micrometer. Four reference stars were taken in each case, situated as symmetrically as possible about the comet. The positions were derived from the Catalogues of the Astronomische Gesellschaft.

Date. 1904.	G.M.T. h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log Δ.	Corr. for Parallax. R.A. s	Dec. ″
Apr. 17	11 21 31	16 56 20·29	44 44 31·9	0·3490	−·20	+1·2
18	11 19 59	16 53 34·02	44 22 5·6	0·3490	−·20	+1·2
19	10 24 14	16 50 48·65	45 57 48·9	0·3490	−·22	+1·5
20	9 25 34	16 47 58·55	46 32 55·8	0·3490	−·24	+1·9
20	10 37 52	16 47 49·39	46 34 45·8	0·3490	−·21	+1·3
21	10 30 41	16 44 46·95	47 10 38·6	0·3490	−·22	+1·3
25	9 52 42	16 31 42·01	49 27 17·9	0·3502	−·23	+1·2
25	10 26 28	16 31 36·97	49 28 5·2	0·3502	−·21	+1·0
26	10 40 1	16 28 3·31	50 0 56·2	0·3510	−·20	+0·8
26	11 15 56	16 27 57·82	50 1 44·0	0·3510	−·17	+0·6
27	9 34 44	16 24 35·68	50 31 9·9	0·3518	−·24	+1·2
27	9 46 59	16 24 33·81	50 31 26·2	0·3518	−·23	+1·1
May 2	9 46 8	16 5 6·89	52 56 22·0	0·3556	−·22	+0·7
2	10 3 52	16 5 3·85	52 56 41·8	0·3556	−·21	+·6
3	10 50 9	16 0 47·21	53 23 33·1	0·3570	−·16	+·2
4	9 34 15	15 56 47·45	53 47 10·3	0·3585	−·22	+·6
5	10 23 15	15 52 21·25	54 11 52·5	0·3599	−·17	+·2
7	11 17 8	15 43 24·72	54 57 2 9	0·3628	−·10	−·1
9	11 8 50	15 34 26·11	55 36 41·0	0·3658	−·09	−·2
11	9 25 11	15 25 35·56	56 10 37·2	0·3692	−·18	+·2
14	10 58 18	15 11 19·15	56 55 35·9	0·3752	−·05	−·3
16	9 59 3	15 2 8·63	57 18 37·2	0·3791	−·10	−·3
18	9 37 40	14 52 52·04	57 37 28·4	0·3842	−·11	−·3
18	9 57 1	14 52 48·23	57 37 35·5	0·3842	−·09	−·3
19	9 45 22	14 48 11·82	57 45 22·7	0·3865	−·09	−·3
25	9 31 20	14 21 10·53	58 10 41·9	0·4008	−·05	−·4
25	9 59 37	14 21 5·37	58 10 43·7	0·4008	−·01	−·4
30	10 13 19	14 0 11·57	58 6 37·2	0·4125	+·05	−·4
30	10 27 44	14 0 9·18	58 6 35·2	0·4125	+·07	−·3
31	9 57 40	13 56 18·02	58 3 30·8	0·4151	+·04	−·4
June 3	10 4 39	13 44 59·99	57 49 59·9	0·4230	+·07	−·3
5	10 19 21	13 37 53·69	57 37 55·7	0·4284	+·10	−·2
10	11 42 59	13 21 41·57	56 58 46·4	0·4417	+·20	+·4
16	10 29 21	13 5 38·76	56 0 52·7	0·4577	+·16	+·2
18	10 32 53	13 0 56·59	55 39 25·3	0·4630	+·17	+·3

Date. 1904.	G.M.T. h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log Δ.	Corr. for Parallax. R.A. s	Dec. ″
June 20	10 6 11	12 56 36·64	55 17 29·9	0·4683	+·16	+·3
20	10 17 24	12 56 35·60	55 17 24·7	0·4683	+·17	+·3
21	10 55 5	12 54 28·31	55 5 50·2	0·4709	+·19	+·6
21	11 9 21	12 54 27·00	55 5 43·4	0·4709	+·19	+·7
27	11 0 22	12 43 37·15	53 56 9·1	0·4862	+·19	+·8
28	10 30 41	12 42 4·62	53 44 36·4	0·4887	+·18	+·7
28	10 43 47	12 42 4·03	53 44 30·2	0·4887	+·18	+·8
29	10 28 16	12 40 34·60	53 32 48·3	0·4912	+·18	+·7
29	10 41 39	12 40 33·80	53 32 42·1	0·4912	+·18	+·8
July 1	10 28 2	12 37 44·72	53 9 10·1	0·4960	+·18	+·8
5	10 28 20	12 32 46·40	52 21 59·5	0·5055	+·18	+·9
6	10 14 45	12 31 40·66	52 10 23·6	0·5079	+·17	+·9
8	10 25 53	12 29 35·57	51 47 1·3	0·5125	+·18	+1·0
9	11 10 18	12 28 36·13	51 35 6·2	0·5147	+·18	+1·3
11	10 27 25	12 26 50·54	51 12 30·7	0·5192	+·17	+1·1
15	10 31 49	12 23 46·24	50 27 36·1	0·5279	+·17	+1·2
18	10 20 35	12 21 52·58	49 54 57·2	0·5341	+·17	+1·2
20	10 57 24	12 20 46·10	49 33 21·8	0·5380	+·16	+1·4
20	11 13 24	12 20 45·76	49 33 15·0	0·5380	+·16	+1·5
21	10 22 59	12 20 16·93	49 23 7·7	0·5399	+·16	+1·3
21	10 36 37	12 20 16·56	49 23 1·5	0·5399	+·16	+1·4
28	9 57 54	12 17 38·04	48 12 59·7	0·5530	+·15	+1·3
28	10 13 11	12 17 37·85	48 12 54·7	0·5530	+·15	+1·3
Aug. 2	9 55 2	12 16 30·33	47 26 21·9	0·5614	+·15	+1·4
4	10 22 49	12 16 12·37	47 8 23·2	0·5646	+·14	+1·6
6	10 0 51	12 15 59·67	46 51 13·9	0·5677	+·14	+1·5
8	9 55 32	12 15 51·69	46 34 29·4	0·5706	+·14	+1·5
12	9 50 57	12 15 48·20	46 2 35·3	0·5761	+·14	+1·5
17	9 45 53	12 16 3·06	45 25 44·1	0·5825	+·13	+1·6
17	10 4 15	12 16 3·16	45 25 37·6	0·5825	+·13	+1·7
18	10 39 1	12 16 8·84	45 18 29·9	0·5837	+·11	+1·8
24	9 33 51	12 16 57·29	44 39 53·1	0·5902	+·12	+1·6
24	9 42 42	12 16 57·43	44 39 51·5	0·5902	+·12	+1·7
26	9 36 35	12 17 19·10	44 28 2·1	0·5921	+·12	+1·7
29	9 0 48	12 17 55·21	44 11 25·5	0·5949	+·12	+1·6
Sept. 3	8 50 11	12 19 5·93	43 46 24·7	0·5988	+·12	+1·6
7	8 44 20	12 20 10·49	43 28 58·7	0·6015	+·12	+1·6
9	9 19 22	12 20 45·13	43 21 2·6	0·6027	+·11	+1·8
17	8 40 32	12 23 13·97	42 55 56·0	0·6064	+·11	+1·7

Date.	G.M.T.	Apparent R.A.	Apparent Dec.	Log Δ.	Corr. for Parallax. R.A.	Dec.
1904	h m s	h m s	° ′ ″		s	″
Sept. 19	7 45 44	12 23 52·66	42 51 14·2	0·6071	+·12	+1·5
20	7 42 26	12 24 12·69	42 49 4·6	0·6074	+·12	+1·5
Oct. 3	7 13 55	12 28 39·55	42 35 54·8	0·6090	+·11	+1 6
12	7 3 2	12 31 42·34	42 43 45·6	0·6076	+·11	+1·7
1905.						
Jan. 17	12 25 0	11 39 30·61	60 33 58·1	0 5471	−·17	+ ·1
25	11 48 7	11 16 58·30	62 19 41·0	0·5489	−·16	·0
Feb. 2	11 33 34	10 50 35·30	63 39 12·9	0·5536	−·14	− ·3
25	11 37 46	9 28 25·59	64 15 16·9	0·5816	+·03	− ·5
Apr. 12	10 21 6	8 3 36·90	56 41 20·8	0·6720	+·12	+ ·2
May 8	10 16 29	7 57 26·30	52 11 12·0	0·7223	+·11	+ ·7

Further details of the observations will be given in the volume of "Greenwich Observations."

Royal Observatory, Greenwich:
1906 *April* 10.

Note on the Effects of Difference of Sea and Air Temperatures on Marine Refraction. By the Rev. William Hall, R.N.

(*Communicated by the Secretaries.*)

Observers who use the sea-horizon are often confronted with a displacement of the visible horizon, which, while not amounting to *mirage*, is nevertheless capable of disturbing their "sights." It would appear that very little exists upon which to base a theory of such abnormal refraction, since none of the recognised text-books of nautical astronomy refer to it. I submit a few suggestions.

My attention was first drawn to the subject in 1897, when, in observing equal altitudes of the Sun for rating chronometers at Ras-el-Tin, Alexandria, I found a remarkable discrepancy in results, and was led to connect it with a *mirage* which occurred intermittently during the day. On the next days I took temperatures of air and of the sandy soil and obtained a series of about seven days' observations—about four sets per day—and tried graphic methods of getting some connection between the true and apparent altitudes. It seemed that a formula of the shape

$$R = a + bT^2$$

was indicated, T being difference of temperatures of air and sand.

My results were retained with a view to further experiment,

but the question was not again attacked until 1899, when some observations were made in latitudes ranging between 65° N. and the Equator. Finally, in 1903, continuous observations in the Mediterranean were made during the summer; there were then sufficient data to warrant analysis.

I suggest now that the temperature difference causes an abnormal refraction of two terms

$$R = f(h) + \phi(T)$$

$f(h)$ depends on h, the height of observer's eye, and is constant for a given height of eye. $\phi(T)$ depends on T, the temperature difference, and on h.

I cannot trace any connection between apparent altitude and the correction, although *à priori* one would expect that $\phi(T)$ would involve also the altitude and would vanish at alt. = 90°. Here, however, I admit there are but few data for high altitudes.

The following formula is given as a subject for criticism in the hope that it may either be proved reasonably correct or else be corrected by other observers :—

$R = \frac{1}{20}$ dip. $+ cT$ (plus to alt. when sea is colder.)

where $c = 11{\cdot}4''$ for height 0 feet

11·7	„	20
12·0	„	40
12·4	„	60
12·0	„	80
11·7	„	100

On this it may be remarked that the formula represents the consensus of upwards of 300 observations, giving due weight to separate results according to their distance from the first average.

A point on which any information would be welcomed is this. The term "$\frac{1}{20}$ dip." *changes sign suddenly* as T changes sign. Thus the function is *discontinuous*. I hope that this may be connected with a "critical angle" as in ordinary refraction of transparent substances, but I am unable to justify my conclusions by theory.

Again, I cannot believe that altitude does not affect the correction. But in all probability the disturbance takes place almost entirely in the line of sight from eye to horizon, and hardly at all in that from eye to Sun. Thus an effect of altitude would be masked by errors of observation.

It remains to add that *time* enters into the question. That is, the warming of the air after sunrise lags behind the warming of the mercury in thermometer. Thus a maximum or minimum effect occurs about 2 P.M.—in general it is a minimum.

But, on the contrary, the shift of horizon is plainly observable when a squall drifts up from windward. And (though this is not

directly applicable) the settling down of fog makes a marked difference, more than is accounted for by the usual corrections given by Bessel.

A provisional table is appended. It must be taken as including seconds of arc, not because these are reliable, but because only thus could advantage be taken of the analysis of many observations.

TENTATIVE TABLE

of correction to altitude due to shift of horizon caused by difference of temperature of sea and air (after applying to mean refraction the corrections for state of barometer and thermometer).

Sea colder, *plus* to alt.

Sea warmer, *minus* to alt.

Arguments :—Height of eye, feet

Difference of sea and air, degrees F.

Diff. Fahr.	Height of Eye, Feet. 0 or 140	20 or 120	40 or 100	60 or 80	Diff. Fahr.	Height of Eye, Feet. 0 or 140	20 or 120	40 or 100	60 or 80
°	′ ″	′ ″	′ ″	′ ″	°	′ ″	′ ″	′ ″	′ ″
1	11	25	31	35	7	1 20	1 35	1 43	1 50
2	23	36	43	48	8	1 31	1 47	1 55	2 2
3	34	48	55	1 0	9	1 43	2 0	2 7	2 15
4	46	1 0	1 7	1 13	10	1 54	2 12	2 19	2 27
5	57	1 11	1 19	1 25	20	3 48	4 6	4 19	4 31
6	1 8	1 23	1 31	1 37	30	5 42	6 3	6 19	6 35

Example :—Sea warmer by 23°, H.E. 30 feet

	°	′	″
for 20		4	12
3			52
	5	4	— to alt.

1906 *March* 15.

On Planetary Inversion. By F. J. M. Stratton, B.A.

I. *Introduction.*

§ 1. The question considered in the following paper is the influence of tidal friction in producing secular changes in the obliquities of the members of the solar system. This subject was suggested by some remarks of Professor W. H. Pickering in his

account of the discovery of *Phœbe*, the ninth satellite of *Saturn*.* Discussing the retrograde direction of its motion in its orbit, he offered as an explanation the theory that the planets originally rotated on their axes in a retrograde direction, and that under the influence of tidal friction their axes of rotation had gradually tilted over into their present positions. If, then, a satellite were thrown off in a very early stage of the planet's evolution it would commence moving in a retrograde direction round the planet. If the oblateness of the planet were very small, or the satellite at a considerable distance from the planet's centre, the plane of the orbit of the satellite would not follow the plane of the planet's equator as it tilted over, but would fall back into a stable position near the ecliptic—a term used in this paper for the plane of the planet's orbit. Such a satellite would remain of the retrograde type exemplified by *Phœbe*. If, however, the satellite were evolved in a later stage of the planet's development (after the planet had greatly contracted and become more oblate), the satellite would move in an orbit whose stable position was almost coincident with the planet's equator, and the satellite would follow the planet's equator. Most of the known satellites of the solar system fall into this class.

Professor Pickering urged in support of this view that the classical nebular hypothesis, according to which the planets were thrown off in the form of rings, required an initial retrograde rotation of the planet and not a direct one, as Laplace assumed. But of recent years Sir George Darwin,† Professor T. C. Chamberlin,‡ and Dr. F. R. Moulton § have adduced strong reasons for discarding the ring-theory, and it would seem that such confirmation as it would undoubtedly have given to this investigation must for the present be disregarded. Though apparently the classical form of the nebular hypothesis cannot now be accepted without considerable modifications I have here followed it in general as regards the history of the planetary sub-systems, and have assumed a planet to be a gradually contracting body, which from time to time may pass through a form of instability, resulting in the evolution of a satellite.

I have also assumed that the determining factor in producing secular changes in the obliquity of a planet has been tidal friction. A justification for this assumption may be found in a statement made by Adams ¶ that no secular alterations of a planet's obliquity can arise from the attraction of its satellites and the Sun. One reason for the further assumption of initial retrograde rotation is that it gives an explanation why the two outermost planets should have an obliquity greater than 90°. It

* *Annals of Harvard College Observatory*, vol. liii. No. III., "The Ninth Satellite of *Saturn*," W. H. Pickering.

† Presidential Address to the British Association, South Africa, 1905, p. 17.

‡ *Journal of Geology*, vol. viii.; and Carnegie Institution, Year Book, No. 3.

§ *Astrophysical Journal*, 1900 February and 1905 October.

¶ See p. 385.

does not seem that under any theory of initial direct rotation their obliquities could have increased past 90° to the values that they appear to have at present. "It must be admitted," Sir George Darwin has said,* "that the Uranian system points to the possibility of the existence of a primitive planet with either retrograde rotation or at least with a very large obliquity of equator." At the same time I should feel considerable hesitation in applying to the inner planets a theory based on the position of the satellites of *Uranus* and *Neptune*, were it not that the remarkable discovery of *Phœbe* seems to compel us to attribute to *Saturn* also a primitive obliquity greater than 90°. And if once the possibility of such a change in the obliquity is conceded in *Saturn's* case the further extension of the theory to the inner planets presents no great difficulty. I must admit, however, that the whole theory is of an extremely speculative nature; many serious difficulties still remain unsolved, such as the extension to heterogeneous planets of the theory of tides in a homogeneous, incompressible spheroid. If, therefore, I am betrayed in my paper into too dogmatic statements I hope it will be understood that they owe their emphatic form solely to a desire to avoid the continual use of the conditional mood. If I state as facts results indicated by the theory, it is not because the hazardous nature of the theory is not recognised, but because it becomes intolerable to continue to insist on the point throughout the whole paper. One word of defence should, perhaps, be added for the rather rough approximations employed from time to time. In treating a hypothesis which is certainly largely removed from the facts it does not seem necessary always to employ the methods of the most exact analysis. All that is required is to obtain the general conclusions to which the hypothesis leads.

§ 2. Leaving these general considerations on one side I proceed to discuss the paper in greater detail. Throughout I follow Sir George Darwin's theory of the action of the tides and I make free use of the results that he obtains in his series of papers in the *Philosophical Transactions* of the Royal Society.† The tides are regarded as a bodily deformation of the planet, which is supposed to be a homogeneous, incompressible, viscous spheroid.

* "On the Secular Changes in the Elements of the Orbit of a Satellite revolving about a Tidally Distorted Planet," *Phil. Trans.* part ii. 1880, p. 886.

† "On the Bodily Tides of Viscous and Semi-elastic Spheroids and on the Ocean Tides upon a Yielding Nucleus," *ibid.* part i. 1879.

"On the Precession of a Viscous Spheroid and on the Remote History of the Earth," *ibid.* part ii. 1879.

"On Problems connected with the Tides of a Viscous Spheroid," *ibid.* part ii. 1879.

"On the Secular Changes in the Elements of the Orbit of a Satellite revolving about a Tidally Distorted Planet," *ibid.* part ii. 1880.

"On the Tidal Friction of a Planet attended by several Satellites and on the Evolution of the Solar System," *ibid.* part ii. 1882.

These papers will hereafter be referred to as "Tides," "Precession," "Problems," "Orbits," and "Evolution" respectively.

Laplace has shown that, under certain conditions, compressibility does not affect the results to any great extent; * and Sir George Darwin has shown in "Evolution" that no serious error is involved in the hypothesis of homogeneity as opposed to heterogeneity, so long as only a surface layer of slight viscosity is affected. In the same paper he has shown that tidal friction can have but very slightly affected the dimensions of the planetary orbits. Hence I shall throughout consider the mean distances of the planets as having their present values. Further, I adopt Sir George Darwin's limitation in considering the eccentricities zero. In adapting his results to my hypothesis I shall of course be unable to reproduce all his reasoning, and I shall have to ask the reader, if unacquainted with his papers, to take my quotations from them as established.

In fitting this theory on to Sir George Darwin's investigations as to the tidal evolution of the Earth-Moon system I have to show that, starting with a planet unattended by a satellite, rotating in a retrograde direction and revolving about the Sun, it is possible to proceed to a state of affairs in which the planet in a highly viscous state rotates some 3,000 times in the course of one revolution about the Sun, and has an obliquity of about 11°. Part II. is therefore devoted to a discussion of the secular changes in the obliquity of a viscous spheroid due to the tidal action of one satellite (the Sun). This is practically the problem dealt with in "Precession," but it is here extended to the case where the obliquity is greater than 90°.

In Part III. I introduce a second satellite, small in mass compared with the planet and much nearer than the Sun, and I discuss the influence of the planet's tides on the inclination of the satellite and on the obliquity of the planet. In Part IV. I extend the last case to admit of several small satellites being present, and I make an application of the theory to the satellites of the solar system. In this part I extend the practical applications of the theory by a discussion of the past changes suggested by the theory in the obliquities of the planets and in the orbits of the satellites. Finally in Part V. I give a brief summary of the results obtained in the foregoing paper and of the general conclusions to which the theory points.

§ 3. I must now explain the notation, which, following Sir George Darwin, I shall use throughout the paper. The method employed is that of the disturbing function, changes in the elements fixing a body's position being obtained by differentiating the function with respect to certain of the other elements expressing the position of the disturbed body. Now we have to determine the perturbations of a satellite under the influence of tides raised by itself, and thus the elements of the satellite's path will enter the disturbing function in two ways, in the expression both of the distorted state of the planet and of the position of the satellite. It is only in this latter sense that we require the

* *Mécanique Céleste*, livre iv. ch. v.

function to be differentiated, and hence we make the following convention : the elements of the tide-raising satellite are to be unaccented letters, those of the disturbed satellite (or, if the effect of the planet is being considered, of the disturbing satellite) are to be accented ; the differentiation is to be made with regard to the accented letters, and after the differentiation the two satellites may, if necessary, be made identical by dropping the accents, or they may be kept distinct.

The following are the symbols employed :—

t = the time.

The suffix $_0$ = the initial value of the symbol to which it is attached.

For the planet we have :

M = mass.

a = mean radius.

e = ellipticity of a meridian section.

w = density of the planet supposed homogeneous.

g' = mean gravity at its surface.

$g = \frac{2}{5}\frac{g'}{a}$.

n = angular velocity of diurnal rotation.

ψ = longitude of vernal equinox measured along the ecliptic —some fixed plane regarded as practically coincident with the planet's orbit—from a fixed point in the ecliptic.

i = obliquity of ecliptic.

χ = angle between a point fixed on the equator and the vernal equinox.

For the tide-raising satellite we have :

c = mean distance.

$\xi = \left(\frac{c}{c_0}\right)^{\frac{1}{2}}$.

Ω = mean motion.

j = inclination of orbit to ecliptic.

N = longitude of node.

ϵ = longitude of epoch.

m = mass.

ν = ratio of planet's mass to satellite's mass $= \frac{\mathrm{M}}{m}$.

$\tau = \frac{3\Omega^2}{2(1+\nu)}$.

For the disturbed (or disturbing) satellite the same symbols apply, but accented.

Further, we have a quantity k such that $\frac{kn}{\xi}$ = the ratio of the moment of momentum of the planet's rotation to that of the orbital motion of planet and satellites round their common centre of inertia. If W is the disturbing function the equation giving the variation of the inclination of the satellite's orbit is

$$-\frac{dj}{dt}=\frac{k}{\xi}\left(\frac{1}{\sin j'}\frac{\partial W}{\partial N'}+\tan\tfrac{1}{2}j'\frac{\partial W}{\partial \varepsilon'}\right) \quad \ldots \quad \ldots \quad (1)$$

The equation for the variation of the obliquity is

$$n\sin i\frac{di}{dt}=\cos i\frac{\partial W}{\partial \chi'}-\frac{\partial W}{\partial \psi'} \quad \ldots \quad \ldots \quad (2)$$

These two equations are quoted from "Orbits."

Before giving Professor Darwin's value for W several fresh definitions must be given. There are present in the disturbing function tides of seven distinct speeds * ($2n-2\Omega$, $2n$, $2n+2\Omega$, $n-2\Omega$, n, $n+2\Omega$, 2Ω), and the tides are retarded by different amounts and are of varying heights. The retardation of the phase of the tide is called its "lag," and its height is expressed as a fraction of the corresponding equilibrium tide in a perfectly fluid spheroid. The following table gives the symbols to be employed for the several tides :—

Tide.	Semi-diurnal.			Diurnal.			Fortnightly.
	Slow.	Sidereal.	Fast.	Slow.	Sidereal.	Fast.	
Speed	$2n-2\Omega$	$2n$	$2n+2\Omega$	$n-2\Omega$	n	$n+2\Omega$	2Ω
Height	F_1	F	F_2	G_1	G	G_2	H
Lag	$2f_1$	$2f$	$2f_2$	g_1	g †	g_2	$2h$

The heights and lags are connected by the following relations :—

$$\left.\begin{array}{c} F_1=\cos 2f_1\ ;\ F=\cos 2f\ ;\ F_2=\cos 2f_2\ ;\ G_1=\cos g_1\ ; \\ G=\cos g\ ;\ G_2=\cos g_2\ ;\ H=\cos 2h \\ \tan 2f_1=2(n-\Omega)/\sigma\ ;\ \tan 2f=2n/\sigma\ ;\ \tan 2f_2=2(n+\Omega)/\sigma\ ; \\ \tan g_1=(n-2\Omega)/\sigma\ ;\ \tan g=n/\sigma\ ;\ \tan g_2=(n+2\Omega)/\sigma\ ; \\ \tan 2h=2\Omega/\sigma \end{array}\right\} (3)$$

where $\sigma=2g'\,a\,\omega/19\nu$.

ν is the coefficient of viscosity of the tidally disturbed layer, and hence we may take f roughly as a measure of the viscosity

* The "speed" of a tide is the coefficient v of t in the trigonometrical term $\cos(vt+p)$ occurring in the expression for the tide's action.

† Note that this is not the same g as the one defined above, p. 378. As this g is an angle and always appears in a trigonometrical form there is no chance of confusion between the two g's.

of the planet ; $f = 10°$ represents a small viscosity, $f = 45°$ an infinite one.

$$\left.\begin{aligned} \text{Let now } \mathrm{P} = \cos\frac{i}{2},\ \mathrm{Q} = \sin\frac{i}{2},\ p = \cos\frac{j}{2},\ q = \sin\frac{j}{2} \\ \varpi = \mathrm{P}p - \mathrm{Q}q\, e^{\sqrt{-1}(\mathrm{N}-\psi)} = \mathrm{P}p - \mathrm{Q}q \text{ expi } \{\mathrm{N}-\psi\} \ * \\ k = \mathrm{Q}p + \mathrm{P}q \text{ expi } \{\mathrm{N}-\psi\} \\ \underline{\varpi} = \mathrm{P}p - \mathrm{Q}q \text{ expi } \{-(\mathrm{N}-\psi)\} \quad \underline{k} = \mathrm{Q}p + \mathrm{P}q \text{ expi } \{-(\mathrm{N}-\psi)\} \end{aligned}\right\} \quad . \quad (4)$$

Then $\mathrm{W} = (\mathrm{W_I} + \mathrm{W_{II}} + \mathrm{W_{III}} + \mathrm{W_1} + \mathrm{W_2} + \mathrm{W_3} + \mathrm{W_0})$ consists of the seven following pairs of terms :—

$\frac{g}{\tau\tau'}\,\mathrm{W_I}$ = slow semi-diurnal term

$= \frac{1}{4}\mathrm{F_I}\varpi^4\underline{\varpi}'^4 \text{ expi } 2\{\chi - \chi' - (\varepsilon - \varepsilon') + \psi - \psi' - f_1\} + \ldots$

$\frac{g}{\tau\tau'}\,\mathrm{W_{II}}$ = sidereal semi-diurnal term

$= \mathrm{F}\varpi^2\underline{k}^2\underline{\varpi}'^2\underline{k}'^2 \text{ expi } 2(\chi - \chi' - \underline{f}) + \ldots$

$\frac{g}{\tau\tau'}\,\mathrm{W_{III}}$ = fast semi-diurnal term

$= \frac{1}{4}\mathrm{F}_2\underline{k}^4\underline{k}'^4 \text{ expi } 2\ \{\chi - \chi' + (\varepsilon - \varepsilon') - (\psi - \psi') - f_2\} + \ldots$

$\frac{g}{\tau\tau'}\,\mathrm{W_1}$ = slow diurnal term

$= \mathrm{G_1}\varpi^3\underline{k}\underline{\varpi}'^3\underline{k}' \text{ expi } \{\chi - \chi' - 2(\varepsilon - \varepsilon') + 2(\psi - \psi') - g_1\} + \ldots$

$\frac{g}{\tau\tau'}\,\mathrm{W_2}$ = sidereal diurnal term

$= \mathrm{G}(\varpi\underline{\varpi} - k\underline{k}(\varpi'\underline{\varpi}' - k'\underline{k}')[\varpi\underline{k}\underline{\varpi}'k' \text{ expi } \{\chi - \chi' - g\} + \ldots]$

$\frac{g}{\tau\tau'}\,\mathrm{W_3}$ = fast diurnal term

$= \mathrm{G}_2(\varpi\underline{k}^3\underline{\varpi}'k'^3 \text{ expi } \{\chi - \chi' + 2(\varepsilon - \varepsilon') - 2(\psi - \psi') - g_2\} + \ldots$

$\frac{g}{\tau\tau'}\,\mathrm{W_0}$ = fortnightly term

$= \frac{3}{2}(\frac{1}{3} - 2\varpi\underline{\varpi}k\underline{k})(\frac{1}{3} - 2\varpi'\underline{\varpi}'k'\underline{k}')$
$+ \frac{3}{2}\mathrm{H}\varpi^2\underline{k}^2\underline{\varpi}'^2\underline{k}'^2 \text{ expi } 2\{\varepsilon - \varepsilon' - (\psi - \psi') - h\} + \ldots$

The second term in each of the above expressions can be obtained from the first by exchanging ϖ with ϖ', $\underline{\varpi}$ with $\underline{\varpi}'$, k with k', and $\underline{k}$ with $\underline{k}'$, and also changing the sign of the exponential expression. This gives the disturbing function to be differentiated, as

* For convenience of printing we write expi for $e^{\sqrt{-1}}$. This abbreviation has been suggested by Professor Turner.

in equations (1) and (2), for the secular variations in the elements of the satellite or the planet.

II. *Secular Changes in the Obliquity of a Viscous Spheroid under the Tidal Action of One Satellite.*

§ 1. This problem has already been discussed by Sir George Darwin in "Precession," and we shall make use of the result he there obtains (p. 474) for the secular rate of variation of the obliquity.

We shall have then

$$\left.\begin{aligned}\frac{di}{dt} = \frac{\tau^2}{gn}\frac{1}{128}\sin i\{&[10\sin 4f_1 - 10\sin 4f_2 + 16\sin 2g_1 - 16\sin 2g_2 - 12\sin 4h]\\ &+\cos i[15\sin 4f_1 - 4\sin 4f + 15\sin 4f_2 + 18\sin 2g_1 - 24\sin 2g + 18\sin 2g_2]\\ &+\cos 2i[6\sin 4f_1 - 6\sin 4f_2 + 12\sin 4h]\\ &+\cos 3i[\sin 4f_1 + 4\sin 4f + \sin 4f_2 - 2\sin 2g_1 - 8\sin 2g - 2\sin 2g_2]\},\end{aligned}\right\} \quad (5)$$

where f, f_1, f_2, g, g_1, g_2, h are determined by equation (3) when some value is assumed for one of them. The extension of Sir George Darwin's work in this paper consists in two things. He deals with values of i from 0° to 90° only, while here i must be taken to vary between 0° and 180°. Again, he has not to consider the question of the variation in the viscosity, or rather in the angle $2f$, as the planet contracts from a nebulous mass and increases its rate of rotation. This point will first be briefly discussed.

If we consider the changes in the viscosity that accompany the evolution of the planet, we find that sufficient data are lacking to enable us to formulate any law on the subject. As will be seen later, the only planets in which the Sun's action on its own tides has been the determining factor throughout in producing changes of obliquity are *Mercury*, *Venus*, the Earth (before the birth of the Moon), and *Mars*. The obliquity of *Mercury* is unknown, that of *Venus* doubtful, different observers giving values as different as 37° and 0°; the obliquity of the Earth when revolving in a highly viscous state at the time of the Moon's birth was about 11°; the present obliquity of *Mars* is nearly 25°. A glance at the details given below will show that, starting with a retrograde rotation, tidal friction would eventually drive the axis of rotation over into a stable position where the obliquity lies somewhere between 90° and 0°. For all but viscosities so large that the semi-annual tide is of greater importance than the diurnal and semi-diurnal tides the stable value of the obliquity will be near 90°. The low obliquities of the Earth and *Mars* can only be obtained if we make the assumption that some such viscosities have obtained in their past history; in picking out a few cases for arithmetical investigation I have been guided rather by this idea than by any supposed law

connecting viscosity with density. The value of the results given lies chiefly in the illustration they afford of the general nature of the graphs for $\frac{di}{dt}$.

§ 2. The accompanying figure gives the form of the graph for $\frac{di}{dt}$ in the following cases :—

$n = 5\mathfrak{N}$	$h = 10^\circ\ '$	$f = 30^\circ\ 36'$
$n = 10$	$h = 11$	$f = 38\ 3$
$n = 20$	$h = 12\ 30$	$f = 41\ 58$
$n = 100$	$h = 15$	$f = 44\ 30\frac{1}{4}$
$n = 1{,}000$	$h = 20$	$f = 44\ 58$
$n = 10{,}000$	$h = 22\ 30$	$f = 44\ 59\frac{3}{4}$

From the figure we see fairly satisfactorily that for varying values of $\frac{n}{\mathfrak{N}}$ and for different viscosities we may make $\frac{di}{dt}$ zero for any value of i we like between 0° and 90°; also, since in passing through this zero value $\frac{di}{dt}$ becomes negative as i increases, the obliquity will there have a stable value, but not at 0° nor at 180°. Starting, then, with a sphere of slight viscosity, with an obliquity of 180°, we shall have initially a decrease of obliquity, and in any case the obliquity will eventually become less than 90°. But as the sphere contracts its viscosity increases, and if it does so in such a way that the effect of the semi-annual tide steadily increases relatively to that of the diurnal and semi-diurnal tides, then, when $\frac{n}{\mathfrak{N}}$ has attained a large value, the sphere will rotate as though almost rigid for these latter tides and the stable position of the obliquity will be a very small angle. If, however, after the planet reaches this position the viscosity still increases for the bodily tides, but an ocean of much smaller viscosity forms upon the surface, then the effect of the bodily tides may become negligible, and under the influence of the slightly viscous ocean tides the planet may gradually tilt over till it reaches a position of stability with a fairly high obliquity.

Thus we may conceive that *Mercury* has tilted over into a position of small obliquity and that it never had oceans of sufficient magnitude to tilt it back. *Venus* may or may not have been seriously affected by ocean tides after reaching a position of low obliquity under the influence of bodily tides. Here observation leaves it open for one to hold either view. The Earth has in its later history been affected by the presence of the Moon, and therefore it need not be considered in this section. *Mars* may never have tilted over so far as the Earth did, owing to its

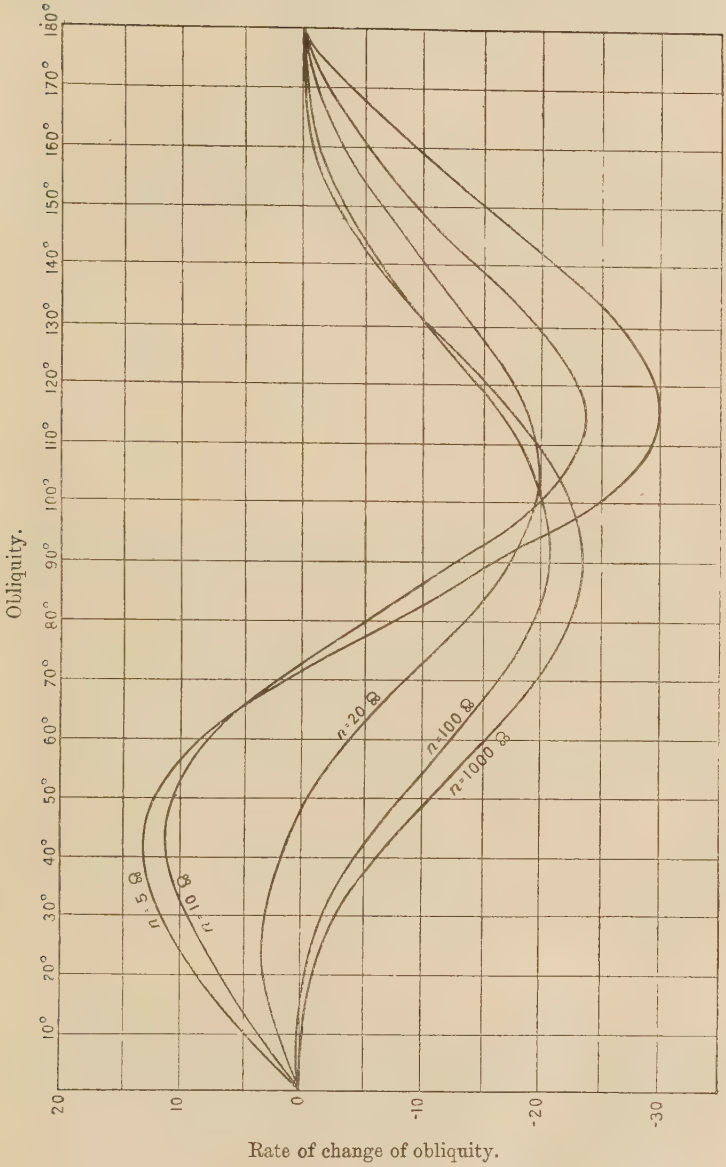


Diagram showing change of obliquity of a planet due to solar tides.

longer period of revolution, and it may be already on the way back, under the influence of ocean tides, to a position of stability with its axis of rotation almost in the plane of its orbit. The other planets will be considered in Part IV.

III. *The Tidal Action of Two Satellites.*

§ 1. When there are two satellites, one of which we distinguish as the Sun, setting up tides on a planet and acting on their own and each other's tides, we have to distinguish four sets of terms in the disturbing function which gives the perturbations of the planet and the satellite :—

(i) when the tide-raiser is the satellite and the disturber is the satellite,
(ii) when the tide-raiser is the Sun and the disturber is the satellite,
(iii) when the tide-raiser is the satellite and the disturber is the Sun,
(iv) when the tide-raiser is the Sun and the disturber is the Sun.

For perturbations in the planet all four sets of terms exist, but for perturbations in the satellite we alter "disturber" into "disturbed body," and notice that we have only to consider cases (i) and (ii).

Now let τ as defined on p. 378 refer to the satellite and let τ' refer to the Sun. Then, in considering the changes in the satellite's inclination given by equation (1), we derive from the disturbing function two sets of terms corresponding to cases (i) and (ii) above. These consist of functions of i and j of somewhat the same type as that given in equation (5).* But they have as coefficients the factors $\frac{k}{\xi}\frac{\tau^2}{g}$ and $\frac{k}{\xi}\frac{\tau\tau_s}{g}$ respectively, and these factors are so small for all the satellites of the solar system as to render these terms in the disturbing function negligible compared with certain other terms yet to be mentioned.

For the disturbing function (†)

$$\Omega = K\cos^2\gamma + K'\cos^2\gamma'$$

where γ, γ' are the angles made by the satellite's orbit with the planet's orbit and equator respectively, from which, according to the ordinary dynamical theory, the secular position of the satellite's orbit (*i.e.* the satellite's proper plane, round the pole of which the pole of the orbit describes an ellipse) is derived, gives

* The exact form of the expression derived from case (i) is given in "Orbits," pp. 740, 746.

† See Tisserand, *Mécanique Céleste*, vol. iv. ch. vi. s. 37.

some secular terms in the expression for $\frac{dj}{dt}$. These give the following equation for the position of the satellite's proper plane :

$$\mathrm{K} \sin 2j = \mathrm{K}' \sin 2(i-j),$$

or, as it may be written,

$$\tan 2(i-j) = \frac{\mathrm{K} \sin 2i}{\mathrm{K}' + \mathrm{K} \cos 2i} \quad \text{or} \quad \tan 2j = \frac{\mathrm{K}' \sin 2i}{\mathrm{K} + \mathrm{K}' \cos 2i}.$$

The addition to the disturbing function of the tidal terms mentioned in the preceding paragraph will not sensibly affect the equilibrium position of the satellite's orbit. For if $(i-j)$ is small compared with i—i.e. if $\frac{\mathrm{K}}{\mathrm{K}'}$ is small—then the numerator of the above expression for $\tan 2(i-j)$ should be diminished by a quantity of the order of $\left(\frac{a}{c}\right)^5 \mathrm{K} \sin 2i$, and the denominator increased by a quantity of the order of $\frac{a^3}{ec_s^3} \mathrm{K}'$, where a is the planet's mean radius, c, c_s the satellite's and the Sun's mean distances, and e is the planet's oblateness. These alterations will produce a quite insensible diminution in the angle $(i-j)$; in the other case, where $\frac{\mathrm{K}'}{\mathrm{K}}$ is near unity or small, the additional terms due to tidal action are still less sensible than in the above case. In all cases, then, tidal action may be neglected in finding the secular position of a satellite's orbit, which is found by the usual gravitational methods; it is different, however, when we consider the obliquity of a planet.

§ 2. Adams has shown * that the obliquity of a planet is only altered by periodic terms under the attraction of its satellites and the Sun, so that we must look to other causes for secular changes in the obliquity. This being the case it seems fair to assume that the determining factor in the obliquity of a planet is tidal friction, and that is, in fact, the fundamental assumption on which this whole paper is based. For perturbations in the planet we have all the four sets of terms arising from the cases discussed on p. 384 ; as they have respectively the coefficients $\frac{\tau^2}{g}, \frac{\tau\tau_s}{g}, \frac{\tau\tau_s}{g}, \frac{\tau_s^2}{g}$ we see that we need take into consideration the tidal effect of the satellite, only when $\frac{\tau}{\tau_s}$ is large or near unity. When $\frac{\tau}{\tau_s}$ is small the planet moves as though under solar tidal friction alone, in the manner discussed in Part II. Now, except for the Moon, in all cases where $\frac{\tau}{\tau_s}$ is finite or large we have $\frac{\mathrm{K}'}{\mathrm{K}}$ also

* *Observatory*, 1886 April.

large, and the satellite's orbit has a secular position which passes through the node of the planet's equator on the planet's orbit, and is inclined at a very small angle to the planet's equator. That is to say, that in all cases where we have to take into consideration the presence of a second satellite in discussing the change in a planet's obliquity we may take $(i-j)$ small compared with i, and $N-\psi = 180°$,* for N, ψ are the longitudes on the planet's orbit of the vernal equinox and the ascending node of the satellite's orbit.

In this case $\quad \operatorname{expi}(N-\psi) = \operatorname{expi}\{-(N-\psi)\} = -1$;

therefore for the satellite

$$\varpi = Pp - Qq \operatorname{expi}(N-\psi) = \cos\frac{i}{2}\cos\frac{j}{2} + \sin\frac{i}{2}\sin\frac{j}{2}$$
$$= \cos\tfrac{1}{2}(i-j) = \underline{\varpi}$$

$$k = Qp + Pq \operatorname{expi}(N-\psi) = \sin\frac{i}{2}\cos\frac{j}{2} - \cos\frac{i}{2}\sin\frac{j}{2}$$
$$= \sin\tfrac{1}{2}(i-j) = \underline{k},$$

and therefore, since $(i-j)$ is small, k is small, and it is easier in discussing $\frac{di}{dt}$ to keep k and ϖ in our formulæ to the very last and then to substitute. Taking then the various terms of W from p. 380, and substituting in the equation (2), which we wrote in the form

$$n\frac{di}{dt} = \frac{P^2-Q^2}{2PQ}\frac{\partial W}{\partial \chi'} - \frac{1}{2PQ}\frac{\partial W}{\partial \psi'},$$

we derive sets of terms from the four cases as follows :—

Case (*i*).—Since ψ' occurs only with N′ in the ϖ's and k's, and with ϵ' in the other exponential factors, and since N′, ϵ' only occur in the connection $N'-\psi'$, $\epsilon'-\psi'$ it really facilitates the finding of the result to write :

$$\frac{\partial W}{\partial \psi'} = -\left(\frac{\partial W}{\partial N'} + \frac{\partial W}{\partial \epsilon'}\right)$$

For the sake of brevity in the following work we shall drop the factor $\frac{\gamma^2}{g}$ and let ϵ, N, ϵ', N′ stand for $\epsilon-\psi$, $N-\psi$, $\epsilon'-\psi'$, $N'-\psi'$ respectively, and also omit the second terms in W_1, &c. After differentiation we can drop the accents, as the tide-raiser and disturber are the same body. In this case k', $\underline{k}'$ have the same value as k, $\underline{k}$ or $\sin\frac{1}{2}(i-j)$, while ϖ', $\underline{\varpi}'$ have the same value as ϖ, $\underline{\varpi}$ or $\cos\frac{1}{2}(i-j)$. Since we are only seeking the first approximation we shall only discuss the terms in W, which can give us terms of the lowest possible order, namely, the first, in

* See p. 389.

k, k' ; the next lowest order occurring is the third, and terms of that order may safely be neglected.

Now

$$W_I = \tfrac{1}{4}F_1(\varpi^4\varpi'^4 \operatorname{expi} 2\{\chi-\chi'-(\varepsilon-\varepsilon')-f_1\} + \ldots$$

$$\therefore \frac{\partial W_I}{\partial N'} = \sqrt{-1}qQF_1(-\varpi^7 \operatorname{expi}(-2f_1)+\varpi^7 \operatorname{expi} 2f_1)$$

$$\frac{\partial W_I}{\partial \varepsilon'} = \frac{\sqrt{-1}}{1}F_1(\varpi^8 \operatorname{expi}(-2f_1)-\varpi^8 \operatorname{expi} 2f_1)$$

$$\frac{\partial W_I}{\partial \chi'} = -\frac{\partial W_I}{\partial \varepsilon'}$$

$$n\frac{di_1}{dt} = \frac{1}{2PQ}\left\{2Q^2\frac{\partial W_I}{\partial \varepsilon'}+\frac{\partial W_I}{\partial N'}\right\}$$

$$= -F_1 \sin 2f_1\varpi^7\left(\frac{q}{P}-\frac{Q}{P}\varpi\right)$$

$$= F_1 \sin 2f_1\varpi^7 k.$$

W_{II} must give us as the largest possible term one in k^3 ; it is therefore to be neglected here, as also is W_{III}, which gives terms of the seventh order in k.

$$W_1 = G_1(\varpi^3 k\varpi'^3 k' \operatorname{expi}\{\chi-\chi'-2(\varepsilon-\varepsilon')-g_1\} + \ldots$$

$$\therefore \frac{\partial W_1}{\partial N'} = 2G_1 \sin g_1\varpi^5 k(-3kqQ+\varpi qP)$$

$$\frac{\partial W_1}{\partial \varepsilon'} = 4G_1 \sin g_1\varpi^6 k^2$$

$$\frac{\partial W_1}{\partial \chi'} = -\tfrac{1}{2}\frac{\partial W_1}{\partial \varepsilon'}$$

and

$$n\frac{di_1}{dt} = \frac{1}{2PQ}\left\{\frac{\partial W_1}{\partial N'}+\tfrac{1}{2}\frac{\partial W_1}{\partial \varepsilon'}(3Q^2+P^2)\right\}$$

$$= -G_1 \sin g_1\varpi^5 k\left\{\frac{3k}{P}(q-Q\varpi)-\frac{\varpi}{Q}(q+Pk)\right\}$$

$$= G_1 \sin g_1\varpi^5 k(3k^2+\varpi^2)$$

$$W_2 = G(\varpi\varpi-kk)(\varpi'\varpi'-k'k')(\varpi k\,\varpi' k' \operatorname{expi}\{\chi-\chi'-g\} + \ldots$$

$$\frac{\partial W_2}{\partial N'} = 2G \sin g\,\varpi k(\varpi^2-k^2)^2(-kqQ-\varpi qP)$$

$$\frac{\partial W_2}{\partial \varepsilon'} = 0$$

$$\frac{\partial W_2}{\partial \chi'} = -2G \sin g\varpi^2 k^2(\varpi^2-k^2)^2$$

$$\therefore n\frac{di_2}{dt} = -G \sin g\varpi k(\varpi^2-k^2)^2\{pq+\varpi k(P^2-Q^2)\}\frac{1}{PQ}$$

$$= -G \sin g\varpi k(\varpi^2-k^2)^3.$$

W_3 may be neglected for the same reason as W_{III}, and so too may W_o, for the terms of lowest order derived from it are of the third order in k.

Hence, retaining only the terms of the lowest order in k and restoring the omitted factor, we obtain from this case :—

$$n\frac{di_{(i)}}{dt} = \frac{\tau^2}{g}k\varpi^7(F_1 \sin 2f_1 + G_1 \sin g_1 - G \sin g)$$
$$= \frac{\tau^2}{g}\sin \tfrac{1}{2}(i-j)(F_1 \sin 2f_1 + G_1 \sin g_1 - G \sin g).$$

Case (*ii*).—In this case only W_{II}, W_2, W_o can give secular terms in $\frac{di}{dt}$: here ϖ', k', $\underline{\varpi}'$, $\underline{k}'$ have the same value as in case (i), while

$$\varpi = \underline{\varpi} = \cos \tfrac{1}{2}i$$
$$k = \underline{k} = \sin \tfrac{1}{2}i.$$

The term of highest order of magnitude is the term independent of k' arising from W_2, and that is the only term we shall consider here. It gives :—

$$n\frac{di_{(ii)}}{dt} = -\frac{\tau\tau_s}{g}G \sin g\varpi k(\varpi^2 - k^2)(\varpi'^2 - k'^2)\frac{pq}{PQ}$$
$$= -\frac{\tau\tau_s}{g}G \sin g \cos\frac{i}{2}\sin\frac{i}{2}\cos i$$
$$= -\tfrac{1}{4}\frac{\tau\tau_s}{g}G \sin g \sin 2i.$$

Case (*iii*).—Here we have

$$\varpi = \underline{\varpi} = \cos \tfrac{1}{2}(i-j) \qquad \varpi' = \underline{\varpi}' = \cos \tfrac{1}{2}i$$
$$k = \underline{k} = \sin \tfrac{1}{2}(i-j) \qquad k' = \underline{k}' = \sin \tfrac{1}{2}i.$$

Hence, since the coefficient is the same $\left(\frac{\tau\tau_s}{g}\right)$ as in the previous case we need only consider terms independent of k. Since no such term arises from either W_{II}, W_2, or W_o, the only three parts of W which can give secular terms in this case, $\frac{di_{(iii)}}{dt}$, is negligible compared with $\frac{di_{(ii)}}{dt}$.

Case (*iv*) is the case dealt with in Part II., and the result obtained is given in equation (5).

We may greatly simplify the final expression for $\frac{di}{dt}$ by writing $f = f_1 = f_2$, $g = g_1 = g_2$ throughout. For in the only

case where $\frac{n}{\mathfrak{N}}$ is moderately small, so that the approximation might make a sensible difference, the factor $\sin(i-j)$ is so small as to render the term altogether negligible.

We obtain then for $\frac{di}{dt}$ in the presence of two satellites, one of which is the Sun, the equation

$$\left.\begin{aligned} 2gn\frac{di}{dt} &= r^2 \sin \tfrac{1}{2}(i-j) \sin 4f - \tfrac{1}{4}\tau r_s \sin 2i \sin 2g \\ &+ \frac{r_s^2}{64} \sin i \{-12 \sin 4h + \cos i(26 \sin 4f + 12 \sin 2g) \\ &\quad + \cos 2i \,.\, 12 \sin 4h + \cos 3i(6 \sin 4f - 12 \sin 2g)\} \end{aligned}\right\} \ldots \quad (6)$$

There is one correction to the above result which seems well worth noting. We have tacitly assumed that, because $(N-\psi)$ oscillates through a value 180°, we may take 0 and -1 as the mean values of $\sin(N-\psi)$ and $\cos(N-\psi)$ respectively. This is of course not exact for $\cos(N-\psi)$, since its deviations from -1 are always of the same sign, and we must take as its mean value $(-1+\eta)$. In this case we must write

$$\varpi = \cos \tfrac{1}{2}(i-j) - \eta \sin \frac{i}{2} \sin \frac{j}{2} = \underline{\varpi}$$

$$k = \sin \tfrac{1}{2}(i-j) + \eta \cos \frac{i}{2} \sin \frac{j}{2} = \underline{k}.$$

Then retaining the terms in η of the first order from ϖ, $\underline{\varpi}$, k, $\underline{k}$ we have the following corrections to make: In case (i) the factor $\sin \frac{1}{2}(i-j)$ in the expression for $n\frac{di_{(1)}}{dt}$ is to be replaced by $\left(\sin \tfrac{1}{2}(i-j) + \eta \cos \frac{i}{2} \sin \frac{j}{2}\right)$. In case (ii) . $\left(1 - 2\eta \sin \frac{i}{2} \,.\, \sin \frac{j}{2}\right)$ is to be written for 1. The former correction is sensible for many of the satellites of the solar system, though it does not affect the nature of the results; the latter correction is insensible. The total effect of this correction is a slight increase of the direct as opposed to the cross terms. With these corrections equation (6) should read

$$\left.\begin{aligned} 2gn\frac{di}{dt} &= r^2 \left\{ \sin \tfrac{1}{2}(i-j) + \eta \cos \frac{i}{2} \sin \frac{j}{2} \right\} \sin 4f \\ &\quad - \tfrac{1}{4}\tau r_s \left\{ 1 - 2\eta \sin \frac{i}{2} \sin \frac{j}{2} \right\} \sin 2i \sin 2g + \frac{r_s^2}{64} \sin i \\ &\quad \left\{ \begin{aligned} &-12 \sin 4h + \cos i(26 \sin 4f + 12 \sin 2g) \\ &+ \cos 2i \,.\, 12 \sin 4h + \cos 3i(6 \sin 4f - 12 \sin 2g) \end{aligned} \right\} \end{aligned}\right\} \quad 6\,(a)$$

For practical applications it becomes necessary to extend this

result so as to include the case of several small satellites ; that is done in the following part.

IV. *Extension to the Case where several Small Satellites are Present.*

§ 1. The result given in equation (6) may be easily modified so as to give the value for $\frac{di}{dt}$ in the presence of several satellites. The terms arising from case (i) will be obtained by a simple summation $\Sigma_r \tau_r^2 \sin \frac{1}{2}(i-j_r) \sin 4f$, where r is a suffix denoting one of the satellites. If we consider the mutual action of two satellites (r and p) we obtain from cases (ii) and (iii), replacing τ_s by τ_p, $-\tau_r\tau_p \sin \frac{1}{2}(i-j_r) \sin 2g$, and $-\tau_r\tau_p \sin \frac{1}{2}(i-j_p) \sin 2g$. The action of the satellites on the solar tides is $-\frac{1}{4}\Sigma_r \tau_r\tau_s \sin 2i \sin 2g$, obtained by a simple summation from the previous result. As before, the action of the Sun on the satellite's tides is negligible in comparison. The action of the Sun on its own tides is unaffected, and we get eventually, if r, p signify any two of the m satellites of a planetary sub-system,

$$\left.\begin{aligned} 2gn\frac{di}{dt} = {} & \sum_{r=1}^{r=m} \tau_r^2 \sin \tfrac{1}{2}(i-j_r) \sin 4f - \sum_{\substack{r=1 \\ r\neq p}}^{r=m} \sum_{p=1}^{p=m} \tau_r\tau_p \sin \tfrac{1}{2}(i-j_r) \sin 2g \\ & -\tfrac{1}{4}\sum_{r=1}^{r=m} \tau_r\tau_s \sin 2i \sin 2g + \frac{\tau_s^2}{64} \sin i\{-12 \sin 4h + \cos i(26 \sin 4f \\ & + 12 \sin 2g) + \cos 2i \,.\, 12 \sin 4h + \cos 3i(6 \sin 4f - 12 \sin 2g)\} \end{aligned}\right\} \quad \ldots \quad (7$$

Or if we allow for the correction discussed at the end of the previous Part, this equation should read

$$\left.\begin{aligned} 2gn\frac{di}{dt} = {} & \sum_{r=1}^{r=m} \tau_r^2 \left(\sin \tfrac{1}{2}(i-j_r) + \eta_r \cos\frac{i}{2} \sin\frac{j_r}{2}\right) \sin 4f \\ & -\sum_{\substack{r=1 \\ r\neq p}}^{r=m} \sum_{p=1}^{p=m} \tau_r\tau_p \left(\sin \tfrac{1}{2}(i-j_r) + \eta_r \cos\frac{i}{2} \sin\frac{j_r}{2}\right) \sin 2g \\ & -\tfrac{1}{4}\sum_{r=1}^{r=m} \tau_r\tau_s \sin 2i \left(1 - 2\eta_r \sin\frac{i}{2} \sin\frac{j_r}{2}\right) \sin 2g + \frac{\tau_s^2}{64} \sin i\{-12 \sin 4h \\ & + \cos i(26 \sin 4f + 12 \sin 2g) + \cos 2i \,.\, 12 \sin 4h + \cos 3i \,(6 \sin 4f \\ & - 12 \sin 2g)\} \end{aligned}\right\} \quad 7\ (a$$

§ 2. We proceed to apply this result to the different members of the solar system. *Mercury* and *Venus* have no satellites and they have therefore been already dealt with in Part II. The Earth-Moon system, differing as it does essentially from the other planetary sub-systems, has been already dealt with by Sir George Darwin in "Orbits." The satellites of *Mars* are so very small that for them $\frac{\tau}{\tau_s}$ is small. In fact, estimating their masses on the assumption that they are of the same density as the

satellite of known mass nearest to them in size, *Mimas*, we have the following approximate table :

Satellite.	Mass in Terms of Primary.	$\frac{\tau}{\tau_s}$.
Deimos	·00000001	$\frac{1}{17}$
Phobos	·0000000003	$\frac{1}{11000}$

Thus the cross terms with the Sun are small compared with the purely solar terms, being equivalent to a reduction of the solar term in (7) $\frac{\tau_s^2}{64}$ 12 sin $2g$ sin i cos i to $\frac{\tau_s^2}{64}$ 10 sin $2g$ sin i cos i. The terms arising from the action of the satellites on each other's tides are negligibly small ; for we have in their case an additional factor, sin $(i-j_r)$, which is not large enough to be sensible to observation. *Mars* will move then almost as if unattended by satellites, in the manner indicated in Part II. The position of equilibrium in its case will be with a somewhat smaller obliquity than if no satellites were present.

We proceed to the consideration of the Jovian system. The table corresponding to this system has been compiled from Professor Young's data * and from Tisserand.†

Satellite.	$\frac{\tau}{\tau_s}$.	$i-j$.
Jupiter V.	Very small	Assumed negligible
Io	103	10″
Europa	35	1′ 7″
Ganymede	32	4′ 38″
Callisto	2·9	24′ 12″

while i is taken as 3° 5′.

We may neglect *Jupiter* VI. and VII. They are so small that their $\frac{\tau}{\tau_s}$ is bound to be very small, and they will have no influence on the motion of the planet's axis. We may note that the key to their high inclination does not seem to lie in tidal friction.‡

* *Manual of Astronomy*, Tables I. and II. pp. 583–5.

† *Mécanique Céleste*, vol. iv. p. 62.

‡ "Professor W. H. Pickering has indeed suggested that his theory does account for the high obliquities of *Jupiter* VI. and VII., the planet having left them behind as it tilted over into its present position. Owing, however, to the fact that secular alterations in the positions of the satellites' orbits are produced far more rapidly than in the planet's obliquity, it is hardly possible that satellites can have been left behind in the way suggested. The fixed plane about which a satellite's orbit oscillates will always lie between the planet's orbit and the planet's equator, and the most that the change in the planet's obliquity could do would be possibly to increase the amplitude of

Substituting in (7) we obtain for *Jupiter* :

$$\frac{2gn}{\tau_s^2}\frac{di}{dt} = 1{\cdot}2 \sin 4f - 5{\cdot}7 \sin 2g - 4{\cdot}6 \sin 2g + (\cdot 02 \sin 4f + \cdot 0001 \sin 2g - \cdot 00006 \sin 4h) \quad \ldots \quad \ldots \quad (8)$$

The additional terms on the right-hand side of this equation due to the correction allowed for in 7 (*a*) are certainly less than

$$+ \cdot 53 \sin 4f - 2{\cdot}17 \sin 2g.$$

In any case, then, the predominant terms are those arising from the cross-action of the satellites on each other's and on the solar tides, and they cause the planet's equator to sink steadily down into the ecliptic.

In a similar way we derive from data given in Struve's *Memoir* * the following table for *Saturn's* system :

Satellite.	$\frac{\tau}{\tau_s}$.	$i-j$.
Mimas...	9·58	0
Enceladus	15·4	0
Tethys...	35·8	0
Dione	28·8	0
Rhea	22·7	2′·79
Titan	96·8	37′·2

while $i = 26° 49'$.

Struve does not give the masses of *Hyperion* and *Iapetus*, but if we use Professor Pickering's photometric measurements † of them and of *Phœbe* as some indication of their diameters, and thence obtain their masses by comparison with satellites of about the same size, we obtain for them the following results :

Satellite.	$\frac{\tau}{\tau_s}$.
Hyperion	·005
Iapetus	·015
Phœbe	·00001

It seems, then, safe to omit these three satellites in considering

the oscillations about this fixed plane. This does not seem a sufficient explanation for the phenomenon in question, and some other cause, such as the close coincidence in size of the orbits, must apparently be sought to account for it."

* *Publications de l'Observatoire Central Nicolas*, vol. xi., "Beobachtungen der Saturnstrabanten," Hermann Struve.

† *Annals of the Harvard College Observatory*, vol. xi. p. 269.

changes in the planet's obliquity, and we obtain from (7) for *Saturn*:

$$\frac{2gn}{\tau_s^2}\frac{di}{dt} = 51\sin 4f - 61\sin 2g - 42\sin 2g + (\cdot 17\sin 4f + \cdot 03\sin 2g - \cdot 03\sin 4h) \quad \ldots \quad (9)$$

The additional terms due to the correction already noted have as an outside value $+0\cdot36\sin 4f - 2\cdot14\sin 2g$.

Here again, then, the cross terms prevail and the planet's equator is being slowly driven down into the ecliptic. For *Saturn* and *Jupiter* alike this result seems independent of the viscosity assumed for the planet.

In extending the theory to the systems of *Uranus* and *Neptune* we are handicapped by lack of exact knowledge of the masses of the satellites and of the obliquities of the planets. Using the data as to the satellites' diameters given by Professor Young, and the same assumptions as before as to their densities we obtain the following results:—

Planet.	Satellite.	Mass in Terms of Primary.	$\frac{\tau}{\tau_s}$.
Uranus	Ariel	·0000003	42
	Umbriel	·00000014	8
	Titania	·000009	110
	Oberon...	·0000016	8
Neptune	Lassell's satellite ...	·0004	40,00

A difficulty arises if we try to get some idea of the positions of the equators of these planets, or rather of the secular values of $(i-j_r)$ for their various satellites. For the positions of the equators have not been determined by observations, and the satellites have not yet been observed long enough with sufficient accuracy to enable one to deduce the secular values of their inclinations with any degree of certainty; and naturally the obliquity and oblateness of these planets cannot yet be derived by theoretical means from the motions of their satellites. In view of the great distances of these planets from the Sun it seems tolerably safe to assume a negligibly small value of $(i-j_r)$ for their satellites. In the case of *Uranus* the most likely assumption is that the predominant term is the one arising from the action of the Sun on the satellite's tides; if this is so, then at present the obliquity is increasing from 90° towards 180°. In the case of *Neptune's* satellite we know that η is fairly large, and considering this in conjunction with the large value of $\frac{\tau}{\tau_s}$ we derive from 7 (*a*) with a considerable amount of certainty the conclusion that the obliquity of *Neptune* is at present also increasing towards 180°.

§ 3. (*a.*) We may now inquire in greater detail what this theory points to as the probable past history of the outer

planetary system. With regard to *Jupiter* it requires that when the satellites were first evolved solar tidal friction had already reduced the obliquity below 90°. For otherwise *Jupiter's* obliquity, instead of decreasing, would have increased from 90° and would at present be nearly 180°; and it does not seem possible that *Jupiter* VI. and VII. would have obliquities less than 90° at present, if they had been evolved while *Jupiter* had an obliquity greater than 90°. For *Saturn*, on the other hand, it seems necessary that *Phœbe* should have been evolved while the obliquity was still greater than 90°; possible that *Iapetus* and *Hyperion* were also evolved then, and impossible for *Titan* and the inner satellites to have been existing then. In that case the equation $K \sin 2j = K' \sin 2(i-j)$ connecting i and j would have led to the following state of affairs: When *Phœbe* was evolved its orbit would gradually move down to the ecliptic, as for it $\frac{K}{K'}$ is large. In the case of *Iapetus* $\left(\frac{K}{K'}\right.$ near and rather less than unity$\left.\right)$ the orbit would move from the planet's equator down to the ecliptic through an angle certainly less than $(180°-j)$. *Hyperion* would remain near *Saturn's* equator. As *Saturn's* axis slowly moved, under the influence of the solar tides and practically undisturbed by these three satellites, into the plane of *Saturn's* orbit round the Sun, *Phœbe* would fall more and more closely down to the ecliptic, while *Iapetus* would fall back upon the planet's equator. For only thus could the equation $K \sin 2j = K' \sin 2(i-j)$, $(K \neq K')$ be satisfied for $i = 90°$. *Hyperion* and *Iapetus* then would follow *Saturn's* equator over, while *Phœbe* would settle down with an obliquity near 180°. It is necessary that *Titan* and the other satellites should not be evolved with $i > 90°$, for in that case the cross term given by the action of the Sun on their tides would have been the predominant term, and *Saturn* would have moved down to an obliquity of 180° instead of one of 0°.

The case of *Uranus* seems to require that its satellites were evolved while the planet had an obliquity greater than 90° and less than its present value of 97° 51′. For the expression for $\frac{di}{dt}$ consists of two parts. One part, due to the cross terms, varying with $-\sin 2i$ and driving the obliquity away from 90°, is at present apparently the most important. The other, or solar, part tending to diminish the obliquity, decreases in value as i approaches 90°, but not so rapidly as $\sin 2i$ and not to the value zero. Since the first part prevails now and $i > 90°$, the obliquity can never, since the evolution of the satellites, have been near enough to 90° for the solar part to have been predominant, for then the obliquity would have become less than 90° and have remained so. And, since the obliquity is at present increasing, the satellites must have been evolved while the planet had an obliquity greater than 90° and less than its present value.

Neptune's one satellite too must have been evolved while *Neptune* had an obliquity greater than 90° for it to have ever reached its present position. At present, by its action on its own tides, it appears to be driving *Neptune* towards a position with an obliquity of 180°.

This theory accords well with Sir George Darwin's view * that the outer planets should evolve their satellites at an earlier period in their evolution than the inner ones. But it leaves us with a serious difficulty with regard to time, a difficulty which it is impossible to face properly. For with elements of uncertainty arising from the changing viscosity, density, and rotation period of the planet, with further complications of an unknown nature springing from the birth of a satellite and from an inability to settle at what period in the planet's evolution the satellite's tides will rise into importance, and with the greatest unknown factor remaining in the effects of heterogeneity, any results that might be obtained by integration would have very little value and would form a very inadequate return for the toil and labour involved.

Nevertheless it seems advisable to consider several points closely connected with the time difficulty and more open to examination. For instance, the question arises how far, if the planets tilted over under the influence of tides raised by the Sun, the dimensions of their orbits would be simultaneously altered. From "Evolution" (p. 534) we see that the rotational momentum of any planetary system is so minute compared with its orbital momentum round the Sun, that even the complete tilting over of a planet due to solar tidal friction would not require any appreciable compensating change in its mean distance from the Sun to conserve the angular momentum of the system. A finite change of tilt of the planet's equator is possible with only an extremely small alteration in the planet's mean distance. This is confirmed by a comparison of the value of $\frac{di}{dt}$ with the value of $\frac{1}{c}\frac{dc}{dt}$ derived from "Orbits," equation (70). This shows that for changes of obliquity in the solar system caused by tides set up in the planets we may consider the dimensions of the planets' orbits as fixed.† This conclusion is not affected when we consider the

* "Evolution," p. 532.

† Omitting a variable factor, a function of i, $\frac{1}{c}\frac{dc}{dt}$ is of the same order as $\frac{di}{dt}$ (expressed in circular measure) multiplied by

$$\left\{\frac{\text{the orbital momentum of the planet}}{\text{the rotational momentum of the Sun}} \times \frac{\text{rate of rotation of the Sun}}{\text{rate of rotation of the planet}} \times \left(\frac{\text{radius of planet}}{\text{radius of Sun}}\right)^2 \times \left(\frac{\text{mass of planet}}{\text{mass of Sun}}\right)^2\right\}.$$

At present for the solar system the greatest value of the above expression is about $\frac{1}{200,000,000}$, and it does not seem likely that it has been much greater for any of the planets in the past.

similar effects caused by tides in the Sun ; for so long as the viscosities are small these tides will only give rise to terms in $\frac{dc}{dt}$ negligible compared with those already considered.

Taking the constancy of the mean axes of the planets as established as far as this investigation is concerned, we will now compare the relative size of the couples which the Sun exercises upon the various planets, or rather of the resulting rates of change of obliquity. Omitting the variable factor, arising from the function of i, which is always present in the expression for $\frac{di}{dt}$, and taking the relative rate to be represented by $\frac{\tau^2}{gn} \sin 4f$, further making the arbitrary assumption that the viscosity (supposed small) varies with the density, we obtain the following table :—

Planet.	Rate of Change of Obliquity.
Mercury	222,000,000
Venus	1,440,000
Earth	140,000
Mars	77,200
Jupiter	1
Saturn	$\frac{1}{7}$
Uranus	$\frac{1}{130}$
Neptune	$\frac{1}{4200}$

If, then, tidal friction is in the main responsible for the present obliquities of the planets we are forced by the figures of the above table to one of the following assumptions. Either the outer planets were evolved at periods long anterior to the inner ones, and therefore are relatively more advanced, or else they had initially very different obliquities, the outer ones being more removed from the natural obliquity of 180° than the inner one ;* a third alternative remains, that the inner planets have spent long periods in or near positions of equilibrium, where they have been moving very slowly or not at all ; the result of this would be that they would fall behind the positions which, by comparison with the steadily moving outer planets, they should have reached. Possibly the present state of affairs is due to a combination of these three causes.

There remains one other difficulty in connection with the time required for the working out of the theory, and that difficulty, though an almost necessary accompaniment of any such theory, would be alone sufficient to prevent one from urging its acceptance on dynamical grounds alone. It does not appear

* This might well be the case in the spiral nebular theory outlined by Professor Moulton (*Astrophysical Journal*, 1905 November).

that, for such enormous periods of time as we are here concerned with, our ordinary dynamical equations are of sufficient exactitude to prevent the entrance of some unknown factors, which may profoundly modify the course of the evolution of the system. This difficulty must be regarded as an additional cause for receiving the theory with all reserve.

§ 3. (*b.*) We have seen that the mean distances of the planets from the Sun may be regarded as unaffected by tidal friction. It is of interest to discuss how far, as hinted in "Evolution," and so far tacitly assumed in this paper, the same holds good for the orbits of the satellites. A comparison of the value of $\frac{1}{c}\frac{dc}{dt}$ for a satellite* and of $\frac{di}{dt}$ for a planet shows at once that so long as the satellite remains tidally effective (*i.e.* so long as τ_r is of the same order as τ_s) the satellite's orbit will increase very largely, while i will only vary by an extremely minute quantity. In fact, while a satellite's mean distance may be enlarged by tidal friction so much that the satellite's tides become negligible compared with the Sun's tides, the planet will not appreciably alter its obliquity.† The result of this seems to be that the action of the satellites may be neglected in a discussion of the changes of obliquity of a planet due to tidal friction ; the prevailing term, save only for a short interval, is that due to the action of the Sun on its own tides. If this be so, then on examining the present state of affairs in the solar system we are faced by difficulties which had been previously settled by the action of the satellites. The present small obliquity of *Jupiter*, requiring an almost impossibly great viscosity if explained by solar tidal friction alone, had been regarded as a natural consequence of the tidal action of the satellites. And the large angle through which *Saturn* had tilted since the evolution of *Phœbe* had been looked upon as in great part due to the tidal action of its satellites.

The following considerations may perhaps lead to a solution of these difficulties. It will have been noticed that in the previous discussion of the Saturnian system we omitted the effect of the rings on the planet's obliquity. Now the tidal theory of an annular satellite has been briefly sketched in "Precession" (p. 518), and it has been shown that the resultant couple on a planet is practically the same as is obtained from the cross action of a large number of satellites on each other's tides. While all the secular terms in the expression for the rate of change of the dimensions of the ring vanish a secular change is produced in the planet's obliquity by the tides set up by the ring. This we can express as follows : If a, s, r are suffixes referring to the ring, the Sun,

* Derived from "Orbits," (68) and (73).

† An exception to this is the Moon ; and the true significance of the exceptional nature of the Earth-Moon system pointed out in "Evolution" seems to be that in its case finite changes of obliquity and mean distance accompany one another ; the one does not proceed very much more rapidly than the other.

and any satellite of the planet respectively, then the new terms in $2gn\frac{di}{dt}$ to be added to (7) are

$$\left\{ -\tfrac{1}{2}\tau_a{}^2 \sin \tfrac{1}{2}(i-j_a) - \sum_{r=1}^{r=m} \tau_a\tau_r \left(\sin \tfrac{1}{2}(i-j_a) + \sin \tfrac{1}{2}(i'-j_r)\right) - \tfrac{1}{4}\tau_a\tau_r \sin 2i \right\} \sin 2g. \quad (10)$$

In the case of *Saturn's* rings, if we accept Struve's figures for $i-j_a$, namely, $0''.74$, and Tisserand's avowedly doubtful estimate of their mass as $\frac{1}{1000}$ mass of *Saturn*, we obtain the following approximate value for $\frac{\tau_a}{\tau_s}$:

$$\frac{\tau_a}{\tau_s} = 62700.$$

With this value we obtain from (10) the following terms to be added to the expression for $\frac{2gn}{\tau_s{}^2}\frac{di}{dt}$ for *Saturn* given in (9):

$$-(21600+346000+36300) \sin 2g \text{ or } -404000 \sin 2g.$$

This is, of course, only a very rough approximation, but it is sufficient to show that the terms due to the ring, while of the same sign as those due to the cross action of the satellites, far outweigh them in magnitude. At the same time they are not apparently affected with that transitory nature which renders the satellites' tides ineffective in producing large permanent changes of obliquity in the planet. With the above figures representing the magnitude of the tides set up by the ring the difficulty referred to above disappears for *Saturn*; the further difficulty of the small obliquity of *Jupiter* also ceases to have any force if we assume that *Jupiter's* satellites have in the course of their evolution passed through an annular stage.

We may say, then, that the theory of planetary inversion suggests, but does not absolutely require as a condition for its truth, an annular stage in the history of the satellites of *Jupiter* and *Saturn*. More than this we do not care to state till a more detailed application of the tidal theory has been made to the case of a planet attended by a group of satellites. The very doubtful question whether perturbations in a ring of satellites could ultimately lead to the formation of one or several satellites must also be discussed before the difficulties considered in this section can be removed.

V. *Final Conclusions and Summary of Results.*

The previous part concluded with a mention of one or two difficulties which, unlike the more abstract difficulties connected with time and heterogeneity, may yield to analytical treatment

on further investigation. Some explanation may be required for the appearance of this paper is so incomplete a form as is thus implied, and I may perhaps plead in excuse the desire not to overburden this already long paper with a discussion of the details rather than of the principles involved in the theory of planetary inversion. A further reason for not delaying the publication of this paper any longer may be found in the discussion on this subject between Professor W. H. Pickering and Professor F. R. Moulton, which has been going on in the columns of the *Astrophysical Journal.** And I should like to say a few words about their remarks on the subject.

Stimulated though one may be by Professor Pickering's persevering and successful search for a new satellite of *Saturn*, by his bold and equally successful idea of a retrograde orbit for it, and by his suggested explanation for it, one must be careful not to be misled by the vague terms in which he described his theory. These left ample room for the misunderstanding of the theory which, I believe, has invalidated Professor Moulton's criticism of it. The latter's original misconception led him to some further assumptions, which the above analysis of the theory has shown to be unnecessary, but which fundamentally affect the validity of his criticism.

In conclusion I will sum up briefly the view to which the present application of Sir George Darwin's tidal theory has led me. And first let me give a short summary of the results obtained in the investigation. In Part II. by an extension of the results previously obtained by Sir George Darwin I showed that for a planet moving round the Sun unattended by any satellite the stable position was with an obliquity between 0° and 90°, near 90° for all but large viscosities, and steadily decreasing in value as the viscosity increased. Any planet starting with an obliquity greater than 90° would, under the influence of solar tidal friction, tilt over until its obliquity had decreased below that value ; this would hold good so long as no satellites with large tidal influence compared with the Sun were evolved.

In Part III. I proved that, if a satellite were present of the type that we find in the solar system, tidal friction would have no sensible effect on the position of its orbit. I further obtained the additional terms, due to the second satellite, in the expression for the secular changes in the planet's obliquity. In Part IV. I extended these results so as to admit of several satellites being present, and obtained a formula leading to the following conclusions : The direct terms arising from the action of a satellite on its own tides tend to drive the planet's axis of rotation into the ecliptic. The cross terms arising from the action of the satellites on each other's tides and on the solar tides tend to drive the axis of rotation out of the ecliptic into a

* 1905 December.

position making an angle of 90° with it; these are the predominant terms in most of the planetary sub-systems at present, and thus in the presence of a group of satellites of large tidal influence the stable value for the obliquity will be 180° or 0°, and the planet will move away from an unstable equilibrium position with an obliquity of nearly 90°. One exceptional case exists: if one of the satellites has a tidal influence far in excess of that of any of the others, and if this satellite is not too close to the planet, so that its proper plane does not make too small an angle with the planet's equatorial plane, then the prevailing term may be the one arising from the direct action of this satellite on its own tides, and the equilibrium position will be with the planet's axis of rotation nearly in the ecliptic, or with an obliquity just less than 90°.

In the same part I discussed the application of this theory, if true, to the past and present history of the solar system, and I came to the following conclusion: *Mercury*, *Venus*, and *Mars* have tilted over under the influence of solar tidal friction in accordance with the theory of Part II. Either under the influence of bodily tides of great viscosity they have gained an equilibrium position of small obliquity, or under the influence of ocean tides of small viscosity they are returning to a position of equilibrium with an obliquity of nearly 90°. Observation does not settle which of these two alternatives represents the case of *Mercury* and *Venus*, though for *Mercury* I incline to the former. It seems probable that the latter alternative is required to explain the obliquity of *Mars*. The case of the Earth I have omitted, as already fully dealt with by Sir George Darwin; but it is possible to fit into this theory the initial requirements of his discussion of the evolution of the Earth-Moon system. *Jupiter* must have evolved its satellites after its obliquity had decreased below 90°; partly under their influence it has been driven down towards a stable position of small obliquity, which it has now nearly reached. *Saturn* shed *Phœbe*, and possibly also *Iapetus* and *Hyperion*, while its obliquity was greater than 90°; as under solar tidal influence it passed through the critical position, where its obliquity was 90°, *Phœbe* sank down into the ecliptic in a retrograde orbit, while *Iapetus* and *Hyperion* moved over with the planet's equator. Afterwards the inner satellites were evolved, and under their influence and the influence of the rings *Saturn's* obliquity has steadily diminished—and is still diminishing—towards a small stable value. As seems highly probable for a planet further removed from the Sun, and therefore less likely to have its increasing rotation checked by solar tidal friction, the satellites of *Uranus* were evolved in an earlier stage of its evolution, before its obliquity had decreased to 90°; they have stopped the decrease in obliquity, which would arise from the solar action, and they are now driving *Uranus* back to a stable position with an obliquity of 180°. *Neptune*, with its one satellite of extremely large tidal influence, is being driven towards an equilibrium

position with an obliquity of 180°. I should add that uncertainty as to the data for the satellites of *Uranus* and *Neptune* leaves even the present direction of motion of their equators very doubtful, but that the results above given seem on the whole the most probable.

In the concluding section of this part I gave reason to believe that the effect of the satellite's tides on the obliquity of the planet might only be transient. It seems very possible, from such investigation as I have made into the tidal theory of a system of satellites, that their orbits would be enlarged by tidal friction very rapidly compared with the rate at which the planet would tilt over; thus they would not remain tidally efficient long enough to perceptibly affect the obliquity of the planet. If this should be the case—and it seems probable—then I suggest as the easiest explanation of certain remaining difficulties that the satellites of *Jupiter* and *Saturn* have passed through an annular form at some previous stage in their history. This latter idea is not essential to the successful working out of the theory; at present it is only put forward very tentatively indeed, and as a subject for further research.

Viewed broadly, then, the theory of planetary inversion, though it entails some difficulties of detail, remains a tenable hypothesis. As explained by Sir George Darwin's tidal theory it involves three main assumptions: (1) that the outer satellites of a planet were evolved before the inner ones; (2) that the determining factor producing secular alterations in a planet's obliquity has been tidal friction; and (3) that the time involved in the scheme is not so great as to invalidate the ordinary dynamical equations. A justification for these assumptions may perhaps lie in the satisfactory explanation which the theory affords both of the large obliquities of *Uranus* and *Neptune* and of the presence of a satellite such as *Phœbe*. The secular motions with which the theory is concerned are so extremely slow that it can hardly yet be proved or disproved by reference to the gravitational theory of the motions of planets and their satellites;* the theory would gain some support by the discovery of satellites to *Uranus* and *Neptune* of the same type as *Phœbe*, if their motion were retrograde; it would be overthrown if their motion were direct. The theory remains then at present a speculative hypothesis, which is on the whole well supported by the theory of tidal friction, and which gives the only explanation so far offered for certain facts.

My indebtedness to Sir George Darwin's previous work on the tides has been apparent throughout this paper, but I wish to take this opportunity of thanking him most heartily for the helpful advice and kind encouragement which he has given me whilst writing it.

* This was written before I had seen the paper in which Mr. Cowell successfully explained the apparent secular acceleration of the Sun and Moon by an application of the theory of tidal friction.

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